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SCIENTIFIC LAWS AND SCIENTIFIC OBJECTS IN THE *TRACTATUS* *

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IN a recent article¹ Copi voiced the need for further study of the *Tractatus*, particularly in reference to the concepts of objects, properties, and relations. Urmson's book² also voices this need and indicates that further study of the *Tractatus* is indeed helpful and essential to the understanding of the various doctrines associated with those philosophers who were generally grouped together under the name of Logical Positivism. There is one doctrine or theory, generally accepted by those positivists who wrote anything about the matter, which has not, to my knowledge, received sufficient attention and stands in need of a much more thorough analysis than it usually gets. I refer to the theory of scientific laws—and, presumably, the scientific objects whose names occur in such laws—as being logically constructed models, or schemata, which help us to organise and unify our statements about our experience, or which help 'the investigator to find his way about in reality'. That there is still some confusion concerning the theory is evidenced by Hutten's recent book³ and Alexander's review of it.⁴ I think that some of the difficulties of the theory may be cleared away by using the *Tractatus* as the basic foundation from which the theory is derived. The *Tractatus* offers the only basis upon which the theory, in any of its previously stated forms, can be made both intelligible and consistent. If scientific laws and scientific objects are to be conceived as logically constructed models, then it is not enough to explain

* Received 2. ii. 59

¹ Irvin M. Copi, 'Objects, Properties, and Relations in the *Tractatus*', *Mind*, 1958, 67

² J. O. Urmson, *Philosophical Analysis*, Oxford, 1956

³ E. H. Hutten, *The Language of Modern Physics*, London, 1956

⁴ P. Alexander, *Mind*, 1957, 55

analogically or metaphorically what is meant by calling them such. A derivation from more fundamental principles is also required. Otherwise the theory takes on something of an *ad hoc* character.

In this paper I shall give what seems to me to be the derivation of the 'model' theory from relevant propositions of the *Tractatus*, with a view to rendering the theory internally consistent. I shall not be concerned with the relative merits or demerits of the theory as *the* account of scientific laws and scientific objects. Problems concerning the theory as a whole and its conformity with the actual practices of scientists are not the topic of this paper. Consequently, any objection to the theory which is based on extraneous reasons lies outside the scope of this paper and will not be considered.

Assuming the division of statements found in the *Tractatus*, scientific laws, if statements at all, are either descriptive of reality, or are of a logical nature, or are nonsensical.¹ Any (empirical) proposition belongs to one or more of the sciences, and 'the totality of true propositions is the total natural science (or the totality of the natural sciences)' (4.11).² Since all molecular and general propositions are functions of atomic propositions, and since the truth-value of an atomic proposition is determined by its agreement or disagreement with the fact it pictures, all propositions have an ultimate factual reference and therefore belong to the realm of the natural sciences. It follows, then, that all the propositions of science are descriptive of reality, the descriptions being either true or false. For although the propositions may *exhibit* certain forms or structures, they cannot *say* anything about these forms and are thus descriptive only of possible states of affairs. If proposition 4.11 identifies science with the totality of true propositions, then scientific laws, if they are a part of science, must also be descriptive propositions. Since tautologies say nothing about the world, and therefore do not describe anything, tautologies are not a part of science proper. Consequently, scientific laws cannot be conceived as analytic statements. And if scientific laws say something about the world, as surely they must, then they are not nonsensical statements. Yet if all propositions

¹ The division is neither a strict one nor the only one. Strictly speaking, any statement is either sense or nonsense—yet both types may have uses other than descriptive and exhibitiv. This, however, has no direct bearing on the present paper. As it is with complex propositions, it is *we* who make the divisions (as it were, for the sake of experiment) and uses of linguistic expressions.

² As is customary, all references to the *Tractatus* will be followed by its proposition number written in parentheses.

are complexes of atomic propositions and if atomic propositions are logically independent of one another, then the only propositions that can be implied by a complex proposition are its own constituent propositions. Consequently, if scientific laws be taken as empirical generalisations, then all that they could describe would be facts that have already occurred, and hence they would have no predictive power by means of which significant propositions about the future can be formulated and subsequently verified. Yet by means of scientific laws we can, and do, make predictions which are verified in the future. Moreover, the world is the totality of existing atomic facts, and since atomic facts are logically independent of one another, it makes no sense to speak of general facts as being what empirical generalisations describe. Consequently, it would seem that if scientific laws are general statements, they are not descriptive of the world and are genuine propositions. It follows that either they must be logical constructions from atomic propositions conceived analytically as rules for the formulation of genuine propositions, or else they are mere nonsense statements. If scientific laws be conceived as such rules, then they are prescriptive rather than descriptive and hence have nothing to do with the world that science is describing. If scientific laws be conceived as nonsense statements, then we are hard put to explain how sense can be derived from nonsense.

This, it seems to me, is fairly representative of the kind of confusion which prevails in most accounts of the theory of scientific laws as models, a confusion which results from the incompleteness of these accounts of the theory. On the other hand, it seems that scientific laws are neither empirical nor logical nor nonsensical, and yet, on the other hand, they seem to be empirical and logical and nonsensical. The trouble with the matter is that there are senses in which scientific laws can be understood in both ways. A review of the relevant propositions of the *Tractatus* will show this to be the case.

The sections of the *Tractatus* which deal explicitly with scientific theory and natural law are those which are numbered from 6.3 to 6.372. Generally speaking, there is an implicit distinction between a system and a law. A system is a way in which we can bring the descriptions of the universe into a unified form, and corresponding to the different mesh networks that could be used to bring the description of the white surface with the irregular black spots into a unified form are the different systems of describing the world (6.341). Laws are said to be possible forms of the propositions of science, and 'treat of

the network and not of what the network describes' (6.35). We may take this distinction to be the same as the one that is generally made between theory and law. Thus, in some sense, both systems and laws are descriptive. A system, moreover, is said always to be a general description of the world (6.3432) and to be 'an attempt to construct according to a single plan all *true* propositions which we need for the description of the world' (6.343). It could seem, then, that if the true propositions of science are of various forms and if laws organise these propositions according to their logical forms, then systems are ways of organising laws into a single unified whole. If this is so, then both laws and systems are, in some sense, generalised propositions.

The *Tractatus* admits of two forms of generality: essential (or logical) and accidental (or empirical). Since systems and laws are descriptive, they are not mere logical generalities. Logical propositions are tautologies and thus 'treat' of nothing, whereas systems and laws do 'treat' of something. But neither systems nor laws are, strictly speaking, empirical generalisations. For, since all non-elementary propositions are truth-functions of elementary propositions and can be constructed *a priori* according to successive applications of logical operations, and since what is the case is the existence of atomic facts, it makes no sense to speak of 'complex facts' or 'composite facts', and therefore no sense to speak of general facts which may be described by general propositions. In other words, there is no pictorial relation between an empirically general proposition *per se* and a fact; the pictorial relation holds only for elementary propositions and atomic facts. Indirectly, an empirically general proposition may describe the facts of the world in so far as the constituent elementary propositions that can be inferred from it picture possible atomic facts. But no generalisation is, in itself, a possible description of what is the case. And since all generalisations can, theoretically at least, be constructed *a priori* by the successive application of the negation operation on the values of a variable, generalisations can be conceived as logical sums or logical products of the elementary propositions which are the values of the given variable. As the determination of the values of the variables, *p*, *q*, *r*, etc. is the description of the *propositions* for which these variables stand, so, in a general proposition, the inference of its constituent elementary propositions is the determination of those elementary propositions of the same logical form as the general proposition and the general proposition is a 'description' of the elementary propositions which are its 'values'. For example, the propositional form 'All

x 's are mortal' has as its values such general propositions as 'All cats are mortal', 'All men are mortal', 'All vertebrates are mortal', etc. The general proposition, 'All men are mortal', where 'men' is the variable sign, has for its values such propositions as 'This fat man is mortal', 'This tall man is mortal', 'This snub-nosed man is mortal', etc. So, in this sense, general propositions are descriptive of a certain class of propositions having a certain constant logical form, and in so far as they are descriptive of propositions as propositions, general propositions treat of the symbolism and not of what is symbolised. 'That which is peculiar to the "symbolism of generality" is firstly, that it refers to a logical prototype (*Urbild*, or model), and secondly, that it makes constants prominent' (5.522). A general proposition, then, is to be conceived as a logical prototype or model of a class of elementary propositions of a specific logical form.

Natural laws, then, being general propositions, will have the character of being logical models from which all propositions of certain logical forms can be derived or formulated, and as such are possible forms in which the propositions of science can be stated. As empirical generalisations based on past experience, they are logical sums or logical products of those propositions of certain forms that have been found to be true descriptions of actual states of affairs. But the scope of an empirical generalisation is wider than the propositions describing past and present facts, since a general proposition is a description of *all* its values. 'All men are mortal' refers not only to descriptions of all past and all present individual men but also refers to the description of any man that can exist. Hence a general proposition refers to the descriptions of all possible facts of a certain form. Natural laws, then, have the character of being kinds of shorthand expressions of propositions of a certain logical form found to be true descriptions of facts of a given structure, as well as the character of being logical models in accordance with which we can formulate propositions descriptive of future facts of the same logical structure. But since no future event can be inferred from the past or present events, and since a proposition can imply only its own constituent propositions, a natural law, in its function as a possible form of the propositions of science, is an *a priori* rule in accordance with which we can make significant assertions about states of affairs that we have not experienced—or, as they have so often been characterised, natural laws are licences to make inferences where such inferences are not logically justifiable. We assume or postulate that the natural law—or, rather, any proposition formulated in

accordance with the law—will be a true description of any fact having that particular logical structure. 'The process of induction is the process of assuming the *simplest* law that can be made to harmonise with our experience' (6.363). We bring the possible forms of the descriptions to the true descriptions of our experience and choose the simplest of these forms that we can find in those descriptions as our natural laws. The elementary propositions can be grouped together in various classes according to common forms and may thus be brought under a law, which is the model of the logical form of that class of proposition. The elementary propositions are the descriptions of the facts; the natural law says only how the facts are to be described.

Most accounts of the theory of scientific laws as logical models, if they come this far at all, stop at this point. The usual accounts draw an analogy between scientific laws and propositional functions, give a few illustrations or applications of this metaphor, and that is all. But the analogy should be extended further. Propositional functions not only contain symbols expressing a constant form, but contain symbols expressing variables as well. If scientific laws are to be conceived as being analogous to propositional functions, then the scientific objects that are mentioned in these laws must be conceived as being analogous to the variables of propositional functions. For the structure of a fact is nothing which can stand alone, apart from the form of the objects which combine to make that fact. As the *Tractatus* suggests, it is through the logical apparatus of natural laws that the laws themselves speak of the objects of the world (6.3431), and the 'logical apparatus' cannot be completely exhibited without some account of the variables of the functions. A theory of scientific objects is an essential ingredient in the theory of scientific laws, and such a theory can be worked out in terms of the treatment of formal properties and formal concepts found in the *Tractatus*. I shall attempt to outline such a theory in the next few paragraphs.

There seem to be three senses (not necessarily mutually exclusive) in which the *Tractatus* allows us to 'talk' about 'facts' and 'objects'. All these senses are, under the conditions laid down in the *Tractatus*, nonsensical rather than descriptive. Nonetheless, they do show or exhibit something about facts and objects. First, there is the elucidatory sense in which the *Tractatus* itself speaks of facts and objects. Secondly, there is the sense in which scientific laws, as analogous to propositional functions, say that true propositions, as descriptions of existential facts, may be grouped into classes according to their forms.

Thus, for example, if there be a complete system of laws, this would say something about the structure of the facts so described by this system—i.e. it would at least say that the structure of the facts of the world stood to one another in internal relationships which allow them to be completely described by one general network of forms (which would be that of the system in question). This sense might also be expanded to include such activities as the devising and constructing of artificial language-systems. Thirdly, there is a sense in which formal concepts allow us to speak of 'facts'. It is in this sense that we may speak of scientific objects.

We can speak in a certain sense of formal properties of the objects and atomic facts, or of properties of the structure of facts, and in the same sense of formal relations and relations of structures. . . . An internal property of a fact we also call a feature of this fact. . . . A property is internal if it is unthinkable that its object does not possess it. . . . The existence of an internal property of a possible state of affairs is not expressed by a proposition, but it expresses itself in the proposition which presents that state of affairs, by an internal property of this proposition. . . . The existence of an internal relation between possible states of affairs expresses itself in language by an internal relation between the proposition presenting them (4.122-4.125).

Let aRb be any proposition, where a and b are names and R is the way in which the names are combined. Every part of a proposition which characterises its sense (the possibility of the existence of the atomic fact that it pictures) is called an expression, the ' a ', ' R ', and ' b ' being arbitrary physical signs of the expressions. Expressions are said to be that which propositions may have in common with one another, and an expression is presented by a variable, the values of which are the propositions which contain the expression. For example, xRb determines the class of propositions which may contain the expression for which the variable sign ' x ' stands; ' x is darker than pink' determines the class of propositions which may significantly contain the names for which ' x ' stands. The values of a variable are not determined either by a function or by a class, but rather the variable is whatever values have the form of that expression—i.e. have the formal property expressed therein. Also, the variable deals only with the expressions, and has nothing to do with their meanings—i.e. is a description of the symbols and not of what is symbolised. In the above example, only 'colour names' would be the values of x , and perhaps only 'colour names' of restricted kinds. Thus xRy would exhibit the logical form

of the class of propositions the variable names of which are connected **R**-wise. Since objects have forms (i.e. the possibility of their occurrence in facts), they also have formal properties. The variable symbols in a propositional function are propositional variables standing for certain classes of names of objects possessing a common form 'which may be conceived as a formal property of these values' (4.1271). Objects have formal properties in virtue of which they can combine with one another in order to make facts—analogous to the sense in which physical atoms are said to have valences. Wittgenstein gives space, time, and colour (colouredness) as examples of forms of objects (2.024). Since the ways in which objects combine to make facts are nothing independent of the objects themselves, and since the structure of the fact cannot be named (i.e. is not itself an object) or described (i.e. is not itself a fact), internal properties cannot be asserted by propositions and must be seen in the expressions in which the names of the objects occur. The symbols expressing objects are also said to express formal (internal) properties of objects, an internal property being defined as any property of which 'it is unthinkable that its object does not possess it' (4.123). 'If things (objects) can occur in atomic facts, this possibility must already lie in them' (2.0121). Given, for the sake of an example, that 'red' and 'pink' are objects, it is unthinkable that one does not stand in the internal relation of 'is darker than' to the other, just as it is unthinkable that '3' does not stand in the internal relation of 'is the successor of' to '2', and the symbols expressing these objects also express these internal properties and formal relations of objects, just as '2' and '3' also express internal properties and relations of numbers.

Formal concepts of classes of formal properties expressed by the symbols can now be formed, and although objects themselves cannot be ordered under formal concepts, their symbols will show that they (the objects) fall under formal concepts.

In the sense in which we speak of formal properties we can now speak also of formal concepts. . . . That anything falls under a formal concept as an object belonging to it, cannot be expressed by a proposition. But it is shown in the symbol for the object itself. (The name shows that it signifies an object, the numerical sign that it signifies a number, etc.) Formal concepts cannot, like proper concepts, be presented by a function. For their characteristics, the formal properties, are not expressed by the functions. The expression of a formal property is a feature of certain symbols. The sign that signifies the characteristics of a formal concept is, therefore, a characteristic feature of all symbols,

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whose meanings fall under the concept. The expression of the formal concept is therefore a propositional variable in which only this characteristic feature is constant (4.126).

Since the symbols expressing objects also express formal properties of objects, formal concepts of classes of formal properties expressed by symbols can be formed. For example, just as we can place any numerical sign under the concept of 'Number' and then assign it its place in the numerical series, because the numerical signs exhibit the formal property of being numbers and their internal relations to one another, so names exhibit formal properties in virtue of which they may be the values of a particular variable *and* in virtue of which formal concepts are formed. In the example, ' x is darker than pink', only certain symbols—those presenting certain properties—'satisfy' the variable sign; thus ' x ', in this particular way, signifies a property—or a class of properties—and by doing so, 'shows' that the objects that its values name fall under this particular formal concept. Wittgenstein expressed this as follows:

The propositional variable signifies the formal concept, and its values signify the objects which fall under this concept. Every variable is the sign of a formal concept. For every variable presents a constant form, which all its values possess, and which can be conceived as a formal property of these values. So the variable name ' x ' is the proper sign of the pseudo-concept *object*. Wherever the word 'object' ('thing', 'entity', etc.) is rightly used, it is expressed in logical symbolism by the variable name. For example in the proposition 'there are two objects which . . .', by ' (Ex, y) . . .'. Wherever it is used otherwise, i.e. as a proper concept word, there arises senseless pseudo-propositions. So one cannot, e.g. say 'There are objects' as one says 'There are books'. . . . The same holds of the words 'Complex', 'Fact', 'Function', 'Number', etc. They all signify formal concepts and are presented in logical symbolism by variables, not by functions or classes (as Frege and Russell thought). . . . The formal concept is already given with an object, which falls under it (4.127-4.12721).

Thus, although objects themselves cannot be ordered by formal concepts and propositions cannot assert that such and such objects fall under a particular formal concept, their symbols will show that the objects may fall under certain formal concepts, in the same way that a numerical sign shows, as a feature of itself, that what it signifies will fall under the formal concept of 'Number'. We can in this sense talk of 'objects' ('number', 'fact', etc.) and of formal properties of

objects. For we can take some aspect or property expressed by a class of names, form a concept of it and call it by a 'pseudo-name'. Although we are not actually saying anything about the world (since we are only describing the symbolic scaffolding and not what is symbolised), we are in a sense *showing* something about it—i.e. that its components are characterised by such and such features. It is easily seen from this that it makes no sense to speak of the existence or non-existence of formal concepts.

The formal concepts of 'fact' and 'object' correspond analogically to the variables of the propositional functions which correspond to the scientific laws. Facts and objects may be brought into unity under formal concepts expressing some characteristic features of them. Most of the concepts used in the above 'elucidation' of concepts are formal concepts, and this is one of the chief activities of philosophy. To make propositions about the world—to determine how the world is to be described by determining which set or collection of functions is to serve as laws—is the activity of science. Thus, 'In the proposition a state of affairs is, as it were, put together for the sake of experiment' (4.031). To describe the world correctly, and to determine how this is best done, is the function of the scientist, not the philosopher. To the philosopher is left the task of elucidating the meanings of the concepts used in the description of facts and the elucidating of the sense of propositions by means of showing their syntactical relations to one another. Thus, two main tasks of philosophy are concerned with what subsequent analysts have called formation and transformation rules of language, particularly of the language of science.

Returning to scientific objects, if the description of facts may be brought into unity by scientific laws, and if scientific laws may be conceived as being analogous to propositional functions, the objects which combine to make facts may be brought into unity under formal concepts expressing some characteristic features of objects, and scientific objects may be conceived as being functionally analogous to the variables of propositional functions. If a natural law is to function as a schematic model in accordance with which propositions descriptive of facts can be formulated, then it is inadequate and misleading to say it is nothing more than a mere empty schema of the structure of the states of affairs that the propositions formulated in accordance with the schema picture. For the structure of a fact is nothing over and above the forms of the objects which combine to make that fact, and the forms of objects are to be conceived as formal properties of objects.

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Since the formal concept is expressed by the variable in the function, and since the formal concept is a class of formal properties, the variables in the schemata which are the laws of nature will be indirect expressions of the formal properties possessed by objects by means of which those objects can combine with one another. That the objects do so combine with one another as given in the schemata which are the laws of science is an empirical, and therefore contingent, matter. But in order to unify our true propositions by means of their logical forms into such schemata, it is necessary to order objects by means of their formal properties as signified by their expressions falling under formal concepts. A schema includes symbols for both the logical form of the propositions and the values which constitute the variables. It makes no sense at all to speak of propositional functions as bare empty schemata without explicit consideration of the variable. This is one reason why the *Tractatus* insists that it is through the *whole* logical apparatus of the natural laws that the laws speak of the objects of the world. For the objects are what the names that are the values of the variables stand for, and it is these symbols which have the formal properties that the variable signs express. Consequently, in assuming the simplest law that can be made to harmonise with our experience, we are at the same time assuming the simplest formal concepts that can be made to harmonise with the objects as ordering the names which can appear in the significant propositions formulated in accordance with the scientific law. If a scientific law is a structural model in accordance with which the particular propositions may be formulated, then the scientific objects about which such laws seemingly speak are those parts of such models (or, perhaps, component 'submodels') in accordance with which the values of the variables therein are limited to a particular class of names which the propositions so formulated can meaningfully contain. The natural law puts the facts—or rather, the descriptions of the facts—into the logical form by bringing the various descriptions of the facts into a unified form, but in order to do this it must in some sense determine the formal properties that the objects must possess in order that the propositions formulated in accordance with the law can describe these facts. As Copi suggests,¹ if a fact is an entity that can be described, then the objects that combine to make that fact must possess the formal property of being capable of combining in that particular way. And just as the structure of an atomic fact is the way in which the objects are combined, so the logical form of the

¹ Copi, *op. cit.*, 163

schematic model is the way in which its scientific objects are combined. For example, the form of a law that 'says' that the observed motions of observed bodies traversing observed paths are to be described more accurately in terms of the unobserved motions of unobserved particles traversing unobserved paths will be determined by the formal properties of the particles used. Change the formal properties of the particles used in the description of the events and a change in the law will result. But in whatever way the particle be conceived, the 'particle' itself remains a conceptual model—a formal concept—of a group of properties possessed by that class of entities which is ordinarily called 'material' bodies, and the natural law holds only for those entities that possess those properties. An electron, for example, may be conceived as a model for all those entities, actual and possible, that possess a certain set of field properties, and whenever the name 'electron' occurs in a scientific law, propositions formulated in accordance with that law will be descriptive only of a corresponding set of facts (or set of material properties of facts, depending upon the way in which material properties are conceived), i.e. those facts which are the results of the interaction of the objects that possess the particular set of properties in question. And in the same way as scientific laws, scientific objects have the dual character of being logically constructed models and of being indirect expressions of properties of the objects of the world.

If the above represents an adequate outline of the foundations of the theory of scientific laws and scientific objects as logically constructed models, then it should throw some light upon the difficulties that Alexander finds with Hutten's version of the theory. Alexander's critical comments are restricted to Hutten's conclusions concerning the relations between theories, laws, and hypotheses, since the confusions that he notes result from the ambiguities of these relationships.

Alexander begins by noting a confusion in saying that a theory is at once a description of experiments and a formal calculus or language system. He notes that if a language system is *merely* 'a collection of syntactic and semantic rules', then it cannot be said to describe anything, rules being prescriptive rather than descriptive.¹ Yet, he continues, if a language system does in some sense describe experiments, then Hutten has confused the picture by saying that hypotheses—the quite specific existential statements, or empirical propositions—are both 'part of', or 'are contained within', the theory and are also 'derivable from' the theory. Since Hutten explicitly asserts that a theory consists

¹ Alexander, *op. cit.*, 556

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of two parts—a formal calculus which deals with the purely formal properties of symbols and expressions, and a system of semantic rules which provides the empirical interpretation of the formal calculus—he is to be understood as clearly maintaining that a language system is not *merely* a collection of rules, but contains within its framework the descriptive hypotheses that can be formulated in accordance with these rules. He explicitly asserts that a purely formal calculus contains no assertions about experience, and that is why the semantic rules are necessary for its interpretation in terms of experience. But he also asserts that it is the scientific laws, rather than the theory, which function as the semantic rules, and that the hypotheses are derived from these laws. The confusion becomes all the more pronounced when Hutten speaks of hypotheses as being derived from a theory and as being sentences within a theory, without explaining just how this is to be conceived, or is even possible. Alexander seems to think that the source of the confusion lies in (i) the ambiguity of the expressions, ‘contained within’ and ‘part of’, as used in this context, and (ii) the fact that scientific laws may also function as premisses in a theory. In one sense, Alexander points out, any language system contains, as a part of that language system, all the statements that can be formed according to its rules. He calls this the ‘weak’ sense of specific hypotheses being contained by a language system. But Alexander maintains that, ‘just because it contains them *all* it cannot say any one thing rather than another, which a description must do’.¹ According to him, the correct, or ‘strong’ sense in which a language system may be said to be descriptive of experience is the sense in which its descriptive statements are correct descriptions of certain *actual* states of affairs. As for himself, he thinks ‘that it is normal to regard scientific theories as containing hypotheses in the strong sense and therefore correct to regard them as descriptive’ and that it is incorrect to say that an hypothesis is derived from a theory ‘but correct to say that it was derived from some of the higher-level statements of the theory’.² Thus, for Alexander, a theory, if it is descriptive at all, must be descriptive only of quite specific states of affairs that have happened or will happen, and any description of possible states of affairs that will not also be actualised must not be included in the scope of the theory. I do not think that Alexander has completely removed the confusion by distinguishing between these two senses in which a theory may be said to be descriptive or to contain descriptive statements. It would be closer to the truth

¹ Alexander, *op. cit.*, 556

² *Ibid.*, 557

to explain Hutten's comments concerning hypotheses as being both derived from and contained within a theory as unfortunate and inaccurate ways of expressing his main thesis regarding this matter. Alexander also suggests that this might be the case.

Hutten argues that a theory gives order to the symbols and expressions that we use to describe our experience and that it does this by showing the logical characteristics and interrelatedness of these symbols.¹ Theories are constructed by us and are attempts to systematise our descriptions of actual and possible experience. We might say that, for Hutten, theory construction begins with the descriptions of past experiences which have already been unified to some extent into the various unrelated scientific laws, and attempts to discover, in the possible logical relationships between these laws, a single system in terms of which all actual and possible states of affairs may be described. The scientific laws thus preserve the links that the theory has with the factual world in their function as models for the formulation of hypotheses (the descriptive statements). From this point of view, a theory is a system of laws from which the specific descriptions of the world may be derived or formulated. In so far as a law contains all the propositions that may be derived from it, a theory may be said to contain (in Alexander's weak sense) the specific hypotheses, and in so far as a theory is a system of laws, it is in this sense a general description of the world. But neither theories nor laws can be expected to give us the correct specific descriptions of a particular state of affairs at a given time. The conception of science as an instrument for the calculation of any and every event of this world died with the rigidly deterministic doctrine of scientific materialism. To expect a theory to contain (in the strong sense) the correct description of yesterday's weather at a given location would be like expecting a theological doctrine concerning the creation of individual souls to give the names and dates of each individual soul that ever was and ever will be created. As the *Tractatus* notes, a theory, or a law, is always quite general, there never being any mention of particular entities or particular events (6.3432). All that a law states is that if a given entity or event has such and such characteristics, then its description will take the form of that particular law. In other words, the theory or law only gives us directions concerning the form in which to describe the particular states of affairs of the world, and does not tell us that only such and such states of affairs can possibly occur. To quote the *Tractatus* again, 'At the basis of the whole modern

¹ Hutten, op. cit., 32-3

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view of the world lies the illusion that the so-called laws of nature are the explanations of natural phenomena' (6.371).

From the point of view of being a formal system alone, a theory asserts nothing about the world and therefore is not descriptive (in Alexander's strong sense). From this point of view, a theory is *merely* an uninterpreted symbolic system constructed according to certain formation and transformation rules, and the laws are in this sense analytic, functioning either as syntactical rules or as premisses in the deduction of further symbolic expressions. To use Wittgenstein's example, the unapplied (or uninterpreted) mesh network is purely a geometrical design, and is not a picture of the white surface with the irregular black spots. Even after the network has been applied to the white surface, it does not assert anything about the surface.

Although the spots in our picture are geometrical figures, geometry can obviously say nothing about their actual form and position. But the network is *purely* geometrical, and all its properties can be given *a priori* (6.35).

The specific propositions that do describe the spots on the surface will have certain definite formal characteristics in virtue of which they may be brought together under specific laws—laws that specify that the spots may be 'described' by a particular kind of mesh of a definite fineness—and these laws may be further unified according to some formal characteristics that they have in common into a single plan (the *mesh network*, or theory). The laws become part of a single symbolic system and thus function both as syntactical rules or as premisses *in the symbolic system* and as semantic rules for the formation of the specific propositions needed to describe the facts of the world. Thus, although in one sense it is true that specific propositions are put together for the sake of experiment, Alexander is correct in maintaining that it is misleading to say that they are derived from a theory, but not misleading if we say that they are derived 'from some of the higher-level statements of the theory'. Consequently, Hutten's specific hypotheses are contained within the theory only in Alexander's weak sense and can only improperly be said to be derived from the theory. The more accurate account is that the hypotheses are derived from the natural laws and are contained within the theory only in the weak sense since the theory itself does not determine the truth or falsity of the specific hypotheses that can be formed according to its laws.

The difficulty with Alexander's critical remarks regarding the sense in which a scientific theory may be said to be descriptive (or to contain

descriptive statements) lies in his puzzling conception of what is required in order that a specific hypothesis (a proposition descriptive of a specific state of affairs) be descriptive and yet be 'contained within' the theory. His distinction between the weak and the strong senses of 'contained within' is essentially no more than the distinction between the logical possibility of a particular proposition being true and its actually being true. Any proposition that can be formed according to the rules of a language is contained within that language in the weak sense, but only those propositions that are formed according to the rules *and* are found to be a part of the description of actual states of affairs are said to be contained within that language in the strong sense. But, according to Alexander, a scientific theory must contain the specific hypotheses in the latter sense before it can properly be said to be descriptive. The difficulty of this requirement may be exhibited in the following manner. First, in demanding that scientific theories contain specific hypotheses in the strong sense, Alexander is demanding that theories be both generally and specifically descriptive at the same time. As he says, to be a specific description requires saying one thing rather than another about a specific state of affairs—e.g. 'It rained today', rather than 'It was a dry day today'. If a scientific theory be conceived as containing specific hypotheses in the strong sense, then the theory will—in part, at least—be descriptive of specific states of affairs. But the model theory demands that scientific theories and laws always be general descriptions, and to be a general description requires only that specific descriptions be generalised. Consequently, if scientific theories be conceived as general descriptions, they are descriptive only of the specific descriptions (of a certain logical form), and not descriptive of states of affairs. It follows that if a scientific theory be conceived as a general description, then it cannot also be a specific description as Alexander demands. But his requirement that scientific theories can in some sense 'say certain things and do not say certain other things about, for example, the ways in which bodies move' can be retained if we do not also demand that they make statements about specific bodies and their specific motions. A scientific theory can 'say' that the descriptions of moving bodies may be better organised and unified in terms of this particular set of characteristic properties rather than in terms of that particular set of characteristic properties. In the case of different systems of science 'describing' the same states of affairs, each system only determines how the facts are to be described, and does not determine which specific descriptions will be true and which false,

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come what may. For the same 'true' descriptions of the observed astronomical 'facts' may be derived from both the Copernican and the Ptolemaic theories. The truth-values of the particular descriptive propositions can only be determined by a comparison with the facts of the world, and not by the fact that they can be derived from the theory. The theory is a way of *ordering* or *unifying* knowledge of facts, and is not a way of *determining* new knowledge of facts. According to Alexander's demand, however, a scientific theory should be able to perform both these functions, since the theory is not descriptive unless the descriptive statements derived from it are correct descriptions of actual states of affairs. If, in order for a scientific theory to be descriptive, it must contain the correct descriptions of actual states of affairs, then, in so far as it contains propositions asserting merely possible states of affairs, it cannot be said to be descriptive. According to Alexander, these latter propositions are not to be considered as part of the theory (in the strong sense). In short, for Alexander, a scientific theory, like Locke's particular idea of an abstract general triangle, should be at once general and specific.

It would seem that Alexander's critical comments add up to a possible objection to the entire conception of scientific theories and laws as being descriptive only in a general sense, on the grounds that scientific theories should give us specific descriptions—or even explanations—of the specific facts and events of the external world. But the question as to whether science does, or even should, perform such a function lies outside the intended scope of this paper. My only purpose has been to defend the particular theory under consideration against certain charges of being internally inconsistent.

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EXPLANATION ★

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RECENT literature on the logic of explanation reveals a concern to refine, in some cases to challenge, the deductive model.¹ Those discussions seeking to refine this model are valuable for the philosophy of science.² Some of the challenges are seeking to extend the notion of explanation into history and the social sciences.³ All of these recent discussions tend to concentrate upon the logical relations between empirical statements, laws, and hypotheses. The more general, epistemic features of explanation—the relation between explanation and understanding—are usually assumed but infrequently explored. Such an exploration exposes a more primitive, generic form of explanation of which scientific explanation is only a species. To emphasise this generic form of explanation rather than the scientific paradigm has little importance for the philosophy of the physical sciences, but it is important for those concerned with extending the conception of explanation into history and the social sciences. Moreover, if the primitive form of explanation be recognised, philosophy and science can be seen as two branches of knowledge related by their common explana-

★ Received 18. vii. 58

¹ This model was formulated and published by Karl Popper in 'The Poverty of Historicism' in 1944-45, now printed in book form, London, 1957 (see section 28, especially pp. 122-123). A more extensive and formal statement of this model is found in C. G. Hempel and P. Oppenheim, 'The Logic of Explanation', *Readings in the Philosophy of Science*, ed. by H. Feigl and M. Brodbeck, New York, 1953, pp. 319-353.

² F. A. Hayek, 'Degrees of Explanation', this *Journal*, 1955, 6, 209-226; I. Scheffler, 'Explanation, Prediction, and Abstraction', this *Journal*, 1956, 7, 293-310; N. Rescher, 'On Prediction and Explanation', this *Journal*, 1957, 8, 281-291. Hempel has refined and elaborated his earlier analysis in a recent publication, 'The Theoretician's Dilemma', *Concepts, Theories and the Mind-Body Problem*, ed. by H. Feigl, M. Scriven, and G. Maxwell, Minneapolis, 1958, pp. 37-99.

³ A. Donagan, 'Explanation in History', *Mind*, 1957, 56, 145-165; W. Dray, *Laws and Explanation in History*, Oxford, 1957; J. W. N. Watkins, 'Ideal Types and Historical Explanation', *Readings in the Philosophy of Science*, ed. by H. Feigl and M. Brodbeck, New York, 1953, pp. 723-744; M. Mandelbaum, 'Societal Laws', this *Journal*, 1957, 8, 211-225. N. R. Hanson's recent book attacks the deductive model within the physical sciences. See his *Patterns of Discovery*, Cambridge, 1958.

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tory structures but distinct in virtue of specific differences in the logic of their explanations.

This generic form of explanation, what I shall call *systemic explanation*, finds its roots in man's very first cognitive responses to the stimuli of his environment, responses carried on by perception and transmitted to the conceptual forms of man's thought. This generic explanation found its conceptual expression first in myths, then in cosmological and metaphysical theories. Metaphysics frequently provided the problems for science, but in time the pronounced empirical bent of science abandoned this mode of explanation. The objectivisation of explanation in the deductive model marks the furthest remove of any explanation from this generic, systemic mode. This paper is concerned with extrapolating the generic form of explanation from the physical and social sciences. I have elsewhere suggested how philosophy conforms to the same generic pattern in its theories.¹ My concern in this paper is to direct attention to the *logic* of systemic explanation. A further analysis of the claims of this paper would have to show how systemic explanation arises out of and is grounded in recognition and perceptual processes. Such an analysis would constitute the *epistemology* of explanation. The result of this line of thought is, I think, the separation of explanation from the empirical context of science.

I *The Physical Sciences*

It is perhaps too obvious to need stressing that the term 'explanation' is an epistemological term, but it is worthwhile to remind ourselves of this fact while following some of the implications resulting from it. Just as the term 'meaning' has frequently suffered obfuscation at the hands of those who stress semantics over pragmatics, so 'explanation' has sometimes been solidified into one pattern so rigidly that we have been led to credit the process and form of explanation with an undue preponderance of objectivity. More generally, it has been the fate of many terms in our philosophical vocabulary to become associated with one particular use, e.g. 'clarity' with the Cartesian test of indubitability or the formalised structures of semanticists, or 'empiricism' with the Humean tradition. The specification of the meaning of such terms will always involve some prescriptive preference

¹ J. W. Yolton, 'Philosophical and Scientific Explanation', *The Journal of Philosophy*, 1958, 55, 133-143

and accidental associations, so that the recommendation to enlarge the scope of such terms will always be an imposition upon existing conventions. But the acceptance of such a recommendation need not occur without reflection and judgment concerning its significance; reflection and judging in this case can be controlled by references to previous usages of the term as well as to the current value and function of the extension suggested. Since I am concerned to suggest a wider use of the term 'explanation' than usually prevails—although not a use without its other adherents and, I trust, its cogent reasons—I think it important to start by stressing the epistemological nature of the process of explanation. An analysis of this concept cannot ignore the rôle of the one who understands and accepts explanations. We are certainly correct in separating forms of explanation from understanding in the case of young children, the untrained or uninitiated, since every explanation presupposes and demands for its understanding definite information and the grasping of preliminaries. Thus, many of the explanations of theoretical physics are beyond my comprehension. They are still explanations despite this deficiency on my part, but for all their rigour and precision they would not be explanations were they as unintelligible to all men as they are to me. Like most parts of science, the explanatory part is deemed sound only when it has achieved this kind of independence from particular minds; the reliable explanation is one which can be tested and confirmed by repeated observations and experiments by any sufficiently trained researcher. The objectivity of scientific explanation is commonly contrasted with the irrational subjectivity of emotional or valuational beliefs. In a similar way, an individual's explanation of his behaviour may often stand in sharp contrast with a psycho-analytic explanation. It would be tempting and an act of tolerance to suggest that since the emotional or folklore belief (e.g., an old wives' tale) functions for those who accept it as an explanation of the events concerned, each such belief is an instance of different sorts of explanation, each valid in its own right. But I think it obviously unwarranted and useless to insist that the only criterion for an explanation is that its user takes it as or uses it as an explanation. Such is the minimum requirement only. Thus, the suggested tolerance must be tempered by some further conditions, which I shall try to specify in what follows. But I think it might be suggested here that the recognition of the dependence of the concept of explanation upon one who accepts and understands the explanation, does suggest and allow a scaling of explanations in terms of their

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objectivity, their relative independence from any one particular individual. The scale might run from the objectivity of science to the subjectivity of old wives' tales and even perhaps, to the emotional explanations of the distressed or psychotic individual.

Not only is explanation an epistemological concept relative to understanding in the general way just indicated, but it is also a process which has primarily concerned the philosopher, not in the rôle of explainer but in that of examiner interested in elucidating and understanding the logical structure of explanation and the criteria required to differentiate good from bad explanations. The province of explicating the nature of explanation has been claimed by the logician and philosopher of science. The standard analysis of the logic of scientific explanation has stressed the deductive character of such explanation.¹ Every explanation consists of (a) statements referring to the antecedent conditions of the phenomenon to be explained and (b) statements expressing general laws under which the phenomenon is subsumed as an instance of the law. At least four further requirements are necessary for the deductive model. (1) The explicandum must be a logical consequence of the explicans. (2) The explicans must contain general laws which are necessary for the derivation of the explicandum. (3) The explicans must have empirical content, i.e., be capable of test or experiment. (4) The statements in the explicans must be, these authors say, true. Other suggested requirements for a good scientific explanation are (a) that it be self-consistent and (b) that it simplify what we have to accept, i.e. reduce the number of undeducted laws. The most common and obvious type of scientific explanation fitting the pattern of this logical analysis is the causal explanation, but some writers have argued that teleological explanation such as occurs in biology fits this pattern. Behaviouristic and neural-physiological explanations can also be ranged under the general heading of 'scientific explanations'.

Whether this standard model for the logic of scientific explanation fits the actual working theories and hypotheses of the physical sciences is not a concern of this paper ; this issue has been debated and will continue to be discussed by other spokesmen for science. My concern is to suggest that even in the physical sciences, the explanatory force of theories is independent of the particular logic which they may employ. More basic than the specific form taken by explanatory theories and

¹ Hempel-Oppenheim, *op. cit.*, pp. 320-24. The rest of this paragraph is a summary of this analysis of scientific explanation.

statements, more basic than the relations between these theories and statements and the confirming or disconfirming experiments and experiences, is what Conant refers to as a 'conceptual scheme', in terms of which the known data are formulated and models are constructed. It is the development of conceptual schemes which Conant views as the essential feature of scientific explanatory theories. The importance of the data and observations is not overlooked, but their rôle is primarily one of confirming or falsifying rather than explaining. Explanation is achieved when the data can be fitted into some existing theoretical framework or when the old modes of thought are replaced by new conceptual schemes which comprehend a greater variety of data.¹ Hanson likewise stresses the importance of 'conceptual organisation' in science and the 'theory-laden' aspect of all observation.² Toulmin catches somewhat the same point when he characterises scientific discovery as a function not of the data but of the inferences which are drawn about the data. 'The view of optical phenomena as consequences of something travelling and the diagram-drawing techniques of geometrical optics are introduced hand-in-hand: to say that we *must* regard light as travelling is to say that only if we do can we use these techniques to account for the phenomena being as they are.'³ It is our way of representing the phenomena rather than the information we have about the phenomena that marks the explanatory part of science.⁴ 'The heart of all major discoveries in the physical sciences is the discovery of novel methods of representation, and so of fresh techniques by which inferences can be drawn—and drawn in ways which fit the phenomena under investigation.'⁵

More fundamental still is a general attitude taken towards the task of explanation and what it is that is being explained. Consider the attitudes taken by operationalists, constructionalists, and formalists on the one hand, and by realists on the other. Dingle finds it necessary, in writing of the operationalists, to specify the difference of attitudes towards the world: 'any world that we may contemplate is no longer an independent existence whose nature demands or determines them, but rather a logical construct.'⁶ Later in the same essay, Dingle speaks of the 'ultimate object of science' as 'to find relations between

¹ J. B. Conant, *On Understanding Science*, New York, 1956, p. 106

² Hanson, *op. cit.*, pp. 18, 19ff.

³ S. Toulmin, *The Philosophy of Science*, London, 1953, p. 26

⁴ *Ibid.*, pp. 29-30

⁵ *Ibid.*, p. 34

⁶ H. Dingle, 'A Theory of Measurement', *this Journal*, 1950, I, 5

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the elements of our experience'; and later still of the 'whole collection of things' with which science is concerned as 'a thing to be measured and a means of measurement'.¹ In the same way, Bridgman remarks that for the operationalist 'The two together, object and means of observation or measurement, form an indissoluble union; either without the other is meaningless.'² Bridgman contrasts this view with that of commonsense which is a realist attitude. He explicitly warns against allowing this commonsense realism to colour our scientific thinking. We can no longer speak of 'things existing of themselves in their own right'.³ The conditions for explanation from an operationalist point of view necessitate a realignment of our understanding about what is being explained. The logical form of explanation need not be other than the standard model—our empirical references are now given in the language of operations—but the underlying epistemic conditions have been altered from a general belief in realism to some form of logical construct theory of reality, or at least have been restricted to relations between experiences.

Bridgman warns against the danger of mixing the realist with the operationalist attitudes. It is the prevalence and obstinacy of the realist attitude which has controlled much scientific thinking. The controversy over theoretical entities has been especially controlled in this way: do the theoretical terms of our theory refer to entities or are they only constructs or intervening variables? The shift in attitudes taken towards the explanation arises in the change from ordinary perceptual or observational to these theoretical statements. 'We may fall into serious confusion if we try to mix expressions from the two terminologies. Although it is quite correct to translate the word 'table' in certain contexts by the expression 'set of molecules', it is absurd to say that I now perceive a set of molecules.'⁴ As Dingle remarks, 'the electron has not been observed but conceived in order to express relations between things which are observed, and it is therefore not the same kind of entity as an observed body'.⁵ What is required for the proper use of such transcendent hypotheses is that we be conversant with the rules and usages of the terms or concepts in the

¹ H. Dingle, 'A Theory of Measurement', this *Journal*, 1950, I, pp. 11-12, 23

² P. W. Bridgman, 'The Nature of Some of Our Physical Concepts (I)', this *Journal*, 1950, I, 266

³ *Ibid.*, 269

⁴ W. Kneale, *Probability and Induction*, Oxford, 1949, p. 95

⁵ Dingle, *op. cit.*, p. 24

transcendent language. The transcendent object terminology to which Kneale refers is the language of 'electrons', 'atoms', and 'molecules'. Explanation in terms of such concepts differs from the explanations statable in perceptual language not only in virtue of their reference to non-observables but in relation to the difference of understanding required for their use.

In general, what is required for understanding differs with the type of explanation, differs not only in the content required to be understood: there is a difference of *attitude* as well. It may be hopeless, for example, to try to explain the virtues of racial desegregation to a convinced segregationist: all your arguments might be wasted, all your explanations irrelevant. Your arguments and explanations may in fact be irrelevant or poor but we can only judge this after we have the proper attitude which renders us sympathetic towards your point of view. Similarly, for the convinced behaviourist in psychology, the arguments of the non-behaviourist will not be bad arguments, they may be irrelevant and without effect. A similar change in attitude seems to be required within science when explanations move from macroscopic to microscopic entities. The very notion of an object which is not observable, or observable only indirectly, calls for some special exertions on our part. The user of transcendent hypotheses must conceive of something without sensuous qualities but endowed with some kind of structure, if he means to take his hypothesis in a realist way. The very fact that the entities in transcendent hypotheses are sometimes interpreted as theoretical constructs or intervening variables indicates the clash of attitudes reflected between realist and constructionist: the latter tries to employ the same kind of explanatory techniques throughout science by shifting from real entities to terms in a theory.¹ This shift itself may involve a realignment of the understanding, since the constructionist is requiring us to understand his theory only in terms of logical entities or linguistic terms rather than in terms of objects. The *logic* of scientific explanation may be the same in constructionist or realist terms. Explicans is still related to explicandum in the way specified by the deductive model. But the

¹ In his 'Theoretician's Dilemma', op. cit., Hempel discusses at length the interpretation and rôle of theoretical terms in scientific theory. He is of course opposed to the realist interpretation followed by Kneale. I am not concerned to judge this old and technical dispute. It is illuminating for my purposes if we consider the frame of mind required to follow and interpret scientific theory in each of these readings on the question of theoretical entities.

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epistemic features, usually in the background of explanations, emerge when we shift from one to another kind of explanation within science.

2 *The Social Sciences*

The data of the physical sciences are, for purposes of explanation, a function of and are interpreted in terms of some general scheme or framework which renders the data intelligible. Such a claim is easier to accept when applied to the social than to the physical sciences. Human events and persons being the object of investigation, it is easier to achieve agreement in the social sciences that understanding by the historian, anthropologist, or sociologist is an important preliminary to explanation. But even here, the standard deductive analysis of explanation has been defended. In reply to the claim that the model of explanation in the physical sciences has to be modified, at least to include teleological forms of explanation when dealing with purposive and goal-directed phenomena, the standard ploy has been to show how teleological can be assimilated to functional explanation.¹ Teleological explanation, of course, fails to meet the requirements of testability but, so it is argued, the language of purpose can be translated into and replaced by the language of function. The functional explanation explains the rôle or function of some item by reference to its place within the system to which it belongs. But testability must always be the ultimate criterion for explanation even in the social sciences. 'A class of phenomena has been scientifically understood to the extent that they can be fitted into a testable, and adequately confirmed, theory or a system of laws; and the merits of functional analysis will eventually have to be judged by its ability to lead to this kind of understanding.'²

The previous section of this paper has admitted that the deductive analysis of explanation embodies the most rigorous and objective form for explanatory theories. But I have also argued that even there, the reaches of the understanding restrict the content (though not the form) of the explanation. Moreover, I have also argued that beneath the logical rigour one can discern the relevance of certain general attitudes

¹ See E. Nagel, 'Teleological Explanation and Teleological Systems', *Vision and Action: Essays in Honor of Horace M. Kallen*, New Brunswick, 1953, 192-223

² C. G. Hempel, 'The Logic of Functional Analysis', in L. Gross, *Symposium in Social Theory*, New York, 1959, section 7, p. 301

or conceptual schemes. Whether we can assimilate the social sciences to the rigorous, objective pattern of explanation operable in the physical sciences, whether indeed such assimilation would be desirable, are questions I do not wish to consider here. What I want to do is to indicate how certain prominent writers in the social sciences cast doubt upon such a programme and then to expose more of that generic feature of explanation which finds its objective limit in physical science, its subjective origins in metaphysics.

We can accomplish both aims by noting a few points made by David Riesman in an important essay on methodology in the social sciences. Riesman admits that the goal of 'a systematically organized body of observations and strict generalizations as taut and impressive as the structure of natural science' may be realisable in sociology, but he feels the need and value for a more intuitive approach.¹ He sees the tight correlation between explaining and understanding while preferring to follow the method of 'Verstehen' in his own work. Such an approach is not divorced from empirical data, although there are no clear-cut tests for determining whether one's understanding of an historical, anthropological, or sociological phenomenon is correct. Even in the case of apparent refutation by data, Riesman thinks it valuable to persist in one's explanation of social events via 'Verstehen' if 'illumination' is achieved.² The point of departure for Riesman's analyses of society is understanding rather than explanation; but if his analyses do embody and communicate any understanding, they also explain, in a less rigorous sense of the term than the standard use in physical science.

Some people have maintained that sociology and anthropology cannot be scientific, most certainly not scientific in the accepted sense. These people have argued that the analysis of societies and of individuals cannot be restricted to the techniques of observation and experimentation characteristic of the physical sciences. The interview techniques of the social psychologist are likewise too 'external' to shed much light upon the motivational and non-behavioural levels of man and his society. What is required, these writers suggest, is a special sort of intellectual and emotional method which enables the anthropologist and the sociologist to understand the people and societies he investigates. Paul Bohannon has put the case for this technique of social understanding with reference to anthropology. The anthropologist's task is two-

¹ D. Riesman, 'Some Observations on Social Science Research', *Individualism Reconsidered*, Glencoe, Illinois, 1954, pp. 469, 470

² *Ibid.*, p. 473

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fold: to understand the thoughts, values, and ways of living of primitive people, and to communicate or translate that understanding to non-primitive people. The latter task is the more comprehensive since successful translation rests upon understanding.

The basic problem of the anthropologist's type of translation is that he must understand, in an exotic language, a situation of social action or of belief or both. He must understand it divided into the categories and clothed in the imagery of the people whom he studies. But he must, when he returns to his own world, realize that his problem is not . . . to make this imagery and these categories available to several sets of symbolic interpretations. Rather, he must set out—in English or some other European language—these very basic categories, and this very imagery, and show that it has meaning in the exotic language by using his own.¹

A double thought process lies behind such translation, thinking in two cultures and devising means of passing from one to the other. Cultural anthropology may be distinguished from physical anthropology by reference to its need for internal understanding. Historians likewise distinguish two aspects of writing history, what Trevor-Roper has recently called 'historical science and historical imagination'.² The function of the latter is to make the past 'fully intelligible to us by enabling us to enter, as it were, into the minds and passions of people who, in some ways, seem different from us.' Historical imagination is like anthropological understanding: it penetrates to 'the mind of the past, to reconstruct the image of its world, to breathe its atmosphere, to remember its accumulated deposit of experience, to share its sudden predicaments, all of which have long since evaporated, and to see them in its terms, not our own'.³

The view here expressed of sociological, anthropological, and historical understanding has been carefully characterised by Popper in section 18 of *The Poverty of Historicism*. Popper does not think the method of 'intuitive understanding' is unique to the social sciences. Even the physicist, he suggests, 'often uses some kind of sympathetic imagination or intuition which may easily make him feel that he is intimately acquainted with even the 'inside of the atoms'—with even their whims and prejudices'.⁴ But to speak of the 'whims and

¹ P. Bohannon, 'Translation—A Problem in Anthropology', *The Listener*, 1954, 51, 816

² H. Trevor-Roper, 'Historical Imagination', *The Listener*, 1958, 59, 357

³ *Ibid.*, p. 371

⁴ Popper, *op. cit.*, p. 138

prejudices' of atoms is surely to talk analogically. Moreover, Popper admits that we have a more direct knowledge of human events and situations than of physical events. What he argues for is that any hypothesis resulting from such intuitive understanding must be and can be submitted to empirical tests. I am not arguing that this goal of testability, even formulation in terms of a deductive model, is unattainable in the social sciences. I am only calling attention to the effectiveness of the intuitive approach. Whether we stop at the intuitive level or whether we go on to seek confirmation of our explanation, is a matter I would not want to legislate. I am seeking to stress the rôle of understanding in sociological, anthropological, and historical explanation. An explanation of the past, like an explanation of social phenomena whether distant or our own, must be grounded in an understanding of the sort exemplified by Riesman and talked about by Bohannon and Trevor-Roper. The explanatory rôle of such understanding appears when we verbalise it and place it in a context or system of statements. The issue I am raising is whether that system of statements need be testable for it to be explanatory. I am suggesting that while testability is vital for *scientific* explanation, it is not the criterion for *explanation*. The generic feature of all explanation is apparent in the social sciences in the stress upon understanding and context. Watkins has recently discussed this aspect of explanation under the head of 'colligation'. He has distinguished three types of historical explanations: (1) explanation in principle, subsuming an event under some general principle; (2) explanation in detail, by reference to specific persons and specific dispositions; and (3) colligation, 'explaining an event by tracing its intrinsic relations to other events and locating it in its historical context.'¹ Watkins tends to favour explanation in detail, but both (1) and (2) are patterned after scientific forms of explanation. Colligation differs from the other two, and from all forms of scientific explanation recognised by the deductive partisans, in that it is devoid of deductions from principles or other logical inferences. Watkins is hesitant in calling this 'explanation' but agrees to do so by distinguishing between partial and complete explanation. 'An understanding of a complex social situation is always derived from a knowledge of the dispositions, beliefs, and relationships

¹ Watkins, *op. cit.* This discussion is a revised version of a paper originally appearing in this *Journal*, 1952, 3. In talking of colligation as a form of explanation, Watkins is following W. H. Walsh, *An Introduction to Philosophy of History*, London, 1956.

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of individuals. Its overt characteristics may be *established* empirically, but they are only *explained* by being shown to be the resultants of individual activities.'¹ Watkins is still under the influence of the scientific analysis of explanation; but the suggestion of colligation as one possible form of historical explanation and his concern to keep understanding and explaining separate though not necessarily distinct indicate the direction for the expansion of the term 'explanation' beyond its use in the language of science. Understanding and explaining are kept separate in science only by insisting that explanation must involve references to causes, purposes, parts, functions, or constructs in such a way that explicandum and explicans can be shown to stand in a logical relation to each other.

3 *The Logic of Explanation*

In order to extract the logic of explanation from the logic of scientific explanation, we need to emphasise the contextual or systemic aspect of understanding and explaining. Explanation must go on within some particular order system, such that the fact to be explained can be shown to stand in definite relation to other members of the system. In the case of the physical sciences, the explicans is usually deducible from some of the other members of the system, but the deductive relation is not the essential one for explanation. 'A fact or law is explained only when a sufficient knowledge of the system to which it belongs is reached to enable one to interpret the fact or law in terms of that system and as one of the actual members of that coherent and orderly whole.'² Agnes Arber, in *The Mind and the Eye*, defines explanation in biology in this way:

the logical explanation of a phenomenon is the discovery of its own intrinsic place in a nexus of relations, extending indefinitely in all directions. To explain it is to see it simultaneously in its full individuality (as a whole in itself), and in its subordinate position (as one element in a larger whole).³

Hanson insists upon the same structure of explanation in physics and microphysics. 'We have had an explanation of x only when we can set it into an interlocking pattern of concepts about other things, y and z . A *completely* novel explanation is a logical impossibility. It would

¹ Watkins, op. cit., p. 732

² D. S. Robinson, *Principles of Reasoning*, New York, 1936, p. 291

³ A. Arber, *The Mind and the Eye*, Cambridge, 1956, p. 59

be incomprehensible . . .'.¹ Stated more generally, the systemic nature of explanation can be summarised in the words of H. W. B. Joseph: 'We are said to explain, when a conjunction of elements or features in the real, whose connexion is not intelligible from a consideration of themselves, is made clear through connexions shown between them and others.'²

S. Körner has recently put the notion of systemic explanation into more formal terms by making a distinction between a concept and its basis.³ Any assertion, implicitly or explicitly, involves the reference of a concept to its basis. The concept 'to the left of' applies to the spatial relation between any two objects. 'This object is green' entails 'this object is coloured', but it is clear that this entailment does not depend upon the truth of 'this object is green'. The concept of green, in other words, can be isolated from its particular basis and considered in relation to other such isolated concepts. Such is the work of logic. But not all concepts can be thus separated from their bases. Körner wants to say that 'X is a perfect point' cannot be considered without reference to its context or basis in geometry. Thus, 'greenness' involves an independent basis, while 'perfect point' involves a dependent basis. This distinction is important, for it allows him to say that 'what can be imagined can be an independent basis like that which is sensed or perceived' (p. 277). The application of a concept to a basis is another way of speaking of the truth of the concept, but Körner wants to be able to speak of fitting a concept to its basis without reference to the concept's truth or falsity. In some cases, a concept and its contradictory can fit the same basis, just as two different theories can cover the same set of facts. What Körner wants to call 'incomplete fitting' is defined as the case where a concept and its contradictory can fit the same basis or the same context. Complete fitting excludes the contradictory. A descriptive proposition can be defined in Körner's terminology as one whose concept fits its basis completely but whose basis is independent. To say of the table that it is brown excludes saying it is green, although we do not have to have

¹ Hanson, *op. cit.*, p. 54. Cf. p. 72 where he speaks of the physicist as follows: 'He is in search . . . of an explanation of these data; his goal is a conceptual pattern in terms of which his data will fit intelligibly alongside better-known data.'

² H. W. B. Joseph, *An Introduction to Logic*, Oxford, 1931, p. 502

³ S. Körner, 'The Meaning of Some Metaphysical Propositions', *Mind*, 1958, 57, 275-294. Körner's *Conceptual Thinking*, Cambridge, 1955, is an extended application of this distinction to many philosophical problems.

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reference to the table in order to consider the colour brown (279). An explanatory proposition is one whose concept fits its basis incompletely and whose basis is dependent. In general, 'Explanatory concepts . . . entail conjunctions of descriptive concepts', but the converse does not hold, since several different explanatory concepts can fit or entail the same descriptive concept (281). Descriptive concepts are tied to sensation, while explanatory concepts are free.

In other words, the explanatory concept is one which does not exclude its contradictory and one whose meaning-reference, its basis, occurs only in conjunction with its proper concept. The basis is the context or system within which some concept, statement, or theory becomes meaningful; it is the conceptual scheme of Conant or the mode of representation of Toulmin. The historian's use of imagination, the anthropologist's cultural penetration, are ways of trying to reconstruct the basis of the actions and thoughts of the people they write about. Körner's analysis suggests that explanation is not always bound up with truth and falsity. A testable statement or theory must fit its basis completely, excluding contradictory alternatives. But Körner believes there are instances of incomplete fitting which are explanatory. What he has in mind in this particular article are certain metaphysical propositions which are clearly untestable, which incompletely fit their bases.

4 Conclusion

The preceding section has argued that the generic pattern of explanation is its systemic or contextual character. Not deducibility, but intelligibility constitutes the basic feature of the logic of explanation.¹ Understanding and intelligibility are the basic controls operative in every context. Testability and deducibility are the specific controls in the physical sciences, the ideals for many in the social sciences. When the controls of testability and deducibility are set aside for the more generic form of explanation, we do not retreat into some vague common denominator shared by early perceptual responses, mythic, and metaphysical constructions. Understanding is similar in all of these contexts but each context has its own characteristic form of understanding. The final vindication of my claim for a generic explanation present in all these diverse modes of apprehension would

¹ Cf. Hanson, *op. cit.*, p. 18: 'Fundamental physics is primarily a search for intelligibility . . .'.

have to make a detailed analysis of the specific features of each form of understanding. A critique of understanding is a necessary complement to an analysis of explanation.

I think it can be shown that the primitive, generic form of explanation is manifested in the contextual, systemic character of philosophical as well as of scientific theories. But the move from philosophy to science does not enable us to dispense with context and system. Scientific explanations are explanatory only within the framework of the methods and concepts we call 'science'. The essential difference between the systemic explanations of science and of philosophy is that the former are empirical and deductive while the latter are non-empirical. There are many questions and problems shared by philosopher and scientist; many questions can be asked and answered from different points of view, in terms of different contexts. The most difficult part of any question is the determination of its level, its scope of reference, its basis. Clarification of the provinces of philosophy and science must proceed by a careful analysis of the logic of their respective explanatory techniques. But it is also important to notice the similarities, to see that understanding must result before we are entitled to say we have explained, and to record the common systemic bond linking science, social science, and philosophy together.

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THEORIES AND THE DEVELOPMENT OF CHEMISTRY *

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ALL the natural sciences advance by a combination of theorising and observation. The relation between these two activities characterises the dialectic of science and is of perennial interest. The view most often heard at present is that the method of science is hypothetico-deductive. This view, which has been put forward by Popper¹ and followed by Wisdom,² may perhaps be summarised in two phases as follows: (i) Observations are made in order to test hypotheses. This is the characteristic activity of science, which can be carried on only by the hypothetico-deductive method.³ (ii) A theory is tested by applying it to those special or critical cases in which it yields different results from other theories, or different from what we should expect apart from the theory.⁴ Such a test is an attempt to refute the theory, undertaken so that we may not delude ourselves that it is correct if in fact it is not so. If the attempt at refutation succeeds, we conclude that the theory must be amended, and progress will result.⁵

* Received 4. v. 59

¹ K. R. Popper, (a) *The Logic of Scientific Discovery*, London, 1959; (b) *Three Views of Human Knowledge*, in *Contemporary British Philosophy*, ed. H. D. Lewis, London, 1956

² J. O. Wisdom, *Foundations of Inference in Natural Science*, London, 1952, chap. 6

³ Popper, op. cit., (a), chap. 1, and *passim*. Wisdom, op. cit. p. 51: 'The rôle of observations, selected in the light of our hypotheses, is changed: instead of leading to a hypothesis, their function is to test it, and the only way of continuing scientific activity is by means of the hypothetico-deductive system.' Cf. Jevons, *Principles of Science*, London, 1893, chap. 23: 'The investigator . . . uses facts to suggest probable hypotheses; deducing other facts which would happen if a particular hypothesis is true, he proceeds to test the truth of his notion by fresh observations. If any result prove different from what he expects, it leads him to modify or abandon his hypothesis. . . .'

⁴ Popper, op. cit., (a) chap. 10; (b) p. 378: 'A theory is tested not merely by applying it, or by trying it out, but by applying it to very special cases—cases for which it yields results different from what we should have expected without the theory, or in the light of other theories. In other words, we try to select for our tests those crucial cases in which we should expect the theory to fail if it were not true.'

⁵ (a) Wisdom, op. cit., pp. 54, 55: 'What the scientist tries to do is to falsify hypotheses, not to confirm them. . . . But the attempt at refutation may become a

By way of preliminary comment, it should be said that there is no doubt that, in the logician's reconstruction of science, statements of scientific laws and theories can be represented as hypotheses, and the structure of science as hypothetico-deductive. Nor is there any doubt that hypothetical procedures are important in the development of science, insofar as it is systematic; nor can it be denied that theories are refined in the light of their failures. The concentration of attention on hypotheses has led to a much closer analysis of the rôle of theories in science. The account of scientific method as hypothetico-deductive must appeal strongly to the theoretical scientist, who is occupied primarily in the construction and modification of theories. It appeals also to logicians, since only the formally valid logic of falsification is used. It may be questioned, however, whether the experimentalist can be equally satisfied.

If a theory of scientific method claims to be more than a reconstruction of the logical framework of scientific knowledge, then it may be usefully tested by a survey of the procedures actually used in a single branch of science. As Francis Bacon said in another connection, 'we must prepare a natural and experimental history, sufficient and good; and this is the foundation of all'. I propose therefore to enquire how well the hypothetico-deductive scheme describes the procedures of chemistry—an experimental science with a well-developed structure, and one which presents interesting special features.

I *Observations and the testing of hypotheses*

There is no doubt that scientists working in a developed science usually make their observations in the light of some hypothesis, though chance observations cannot be ignored. It is also beyond doubt that many observations are made in order to test some hypothesis. My question here is whether this is the central activity and main purpose of process of refining the hypothesis and finding out new conditions in which it does and does not hold.'

(b) Popper, *op. cit.*, pp. 378 seq. 'Theories are tested by *attempts to refute them*. . . . [The test applied to a theory] is an attempt to refute it; and if it does not succeed in refuting the theory in question—if, rather, the theory is successful with its unexpected prediction—then we say that it is confirmed by the experiment. . . . For it is only in searching for refutations that science can hope to learn and to advance. It is only in considering how its various theories stand up to tests that it can distinguish between better and worse theories and so find a criterion of progress.'

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chemical research. It is necessary first to distinguish in a general way the two types of scientific statement, both of which may be called 'hypotheses', namely theories and empirical laws. By an empirical law I mean a statement that two or more observed factors are correlated (that is, co-present, co-absent, and co-variable); while a theory is an explanation of a set of laws, in the sense that from it may be deduced statements agreeing with the laws. Chemical theories contain terms that are not used in or deducible from the laws, such as 'atom' and 'molecule'.

Empirical laws in chemistry are of two kinds: (a) statements that there are definite kinds of material—hydrogen, sodium chloride, quinine—which we call 'pure substances'; and (b) statements of functional relations, which express the properties of these substances, such as Boyle's law for hydrogen, or the curve of conductivity against concentration for sodium chloride in water at 25°C, or the variation with temperature of the rate of some reaction. Observations are made which test statements of type (a) when analyses are done, and when new substances are prepared (this covers much of the work of organic chemistry); though many substances have been discovered without the aid of a chemical hypothesis, as for instance all those familiar to medicine and the arts before scientific chemistry began. Statements of type (b) can seldom be tested, except when theory is precise enough to predict them, as in the case of Boyle's Law at low pressures. More often theory can predict only what factors will be related, and their exact functional dependence must be investigated experimentally; in this case the observations are not made to test the law, but to find its exact form. It could be claimed that the observations are made to confirm the hypothesis that there is a law connecting the variables that were guessed to be relevant, but in general the exact form of the law is of much greater interest than the mere fact that a law can be formulated.

The more interesting questions concern theories. How far are observations made in order to test theories? It is certainly right to lay stress on the place of theory in a developed science, both in discovery (for theory may predict new observations) and in explanation (for it is theory which confers understanding of laws).¹ New observations are certainly often made in order to test chemical theories; a few examples at random are the theory of complete dissociation of strong electrolytes

¹ Professor Popper's characterisation of science as explanatory rather than instrumental (op. cit., *b*) is certainly applicable to chemistry.

in aqueous solution, the theory that the molecules of ethylene are planar, and the theory that free radicals take part in the reactions that follow the absorption of radiation by solutions. But there are other reasons for which new observations are made. I will omit chance observations, though these can be extremely important; for instance, the discovery of the inert gases began from the observation that the densities of atmospheric and chemically-prepared nitrogen were different. Nor do I wish to lay stress on the vast amount of work in preparative and synthetic chemistry, in which new compounds are prepared, sometimes to support a theory about the structure of the compound, but sometimes simply for the sake of making new compounds. Even apart from work of this type, a great deal of research is done mainly because the phenomenon in question is amenable to experimental investigation¹ and may some day turn out to be of theoretical or practical importance. The results of such investigations are, with respect to existing theories, simply puzzling facts; they constitute a challenge to theorists, rather than a test of any detailed theory. For instance, the conductivities and freezing-points of electrolyte solutions were investigated before ionic theories were developed; the phenomena of conduction in gases, of radioactivity, and of spectra, were studied before anyone had developed the corresponding theories of charged particles, unstable nuclei, and quantised transitions in molecules. The phenomena were there, they were puzzling, and so they were investigated.

The point is that chemists are interested in finding out what goes on in nature, as well as in explaining it.² Moreover they know that their explanation is likely to be incorrect in proportion as their knowledge of facts is inadequate, for experience has taught them that nature is very various and that it is often the unexpected observations that lead to new and better theories. They are interested not only in the phenomena covered by or predicted by current theory, but in any phenomenon that can be investigated. In chemistry at any given time there are always many phenomena that have been observed but

¹ This implies that the factors which may be relevant are believed to be known, possibly with the aid of a theory which predicts them; for example the Phase Rule predicts that a pure liquid will have a definite vapour pressure at a given temperature. In this sense the investigation may be said to test an imprecise hypothesis. But it often happens that theories give no more help than this, and often less, as in the examples below.

² The point is well made in an article by P. Alexander on 'Theory-construction and Theory-testing', in this *Journal*, 1958, 9, 29

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not yet explained. This is especially obvious when a new technique is discovered, such as polarography, or chromatography, or isotope-exchange; it is tried out in all directions, to see what will happen, and usually some of the results are surprising. There may be fields of science—cosmology, perhaps—where the observed facts are relatively few and theories relatively plentiful, so that the great need is new observations to help decide between them. But this is not the case in chemistry, and it would be a mistake to forget the more empirical type of chemical investigation.

It would not be correct, however, to represent chemistry as a science where theory is negligible. Though many chemical phenomena give rise to puzzles, chemistry is a developed science, with a strong body of theory. For most phenomena we have at least a molecular picture, that is, a theoretical hypothesis in terms of atoms and molecules, ions or radicals, which enables us to represent and visualise the situation; and this is a most useful guide to the factors that may be important. For instance, in any study of the mechanism of a chemical reaction, a great deal is known about the molecules concerned in the reaction; what is uncertain is their behaviour at the moment of reaction, and investigation is designed to throw light on this. The molecular model is extremely helpful in suggesting various possibilities, but it is not capable of giving an exact prediction; it is not specific enough, and if it were the calculation would probably be too complex. The observations are undertaken in order to define the model more precisely.

This is a common situation in physico-chemical research. The typical procedure lies, so to speak, between empirical observation and the testing of a theoretical molecular model; it is concerned with measurements designed to give quantitative detail to a molecular model, to make it more exact. This emerges if we consider the main fields of physico-chemical work, which may be summarised broadly as follows.

- (a) The determination of the structures of molecules, by spectroscopic or diffraction methods.
- (b) Thermodynamic investigations, on physical or chemical equilibria; for example, measurements of solubility, vapour pressure, equilibrium constant, electromotive force of cells.
- (c) Kinetic investigations, on the rates of physical or chemical change, particularly on rates of chemical reactions.

In each of these fields the chemist begins his investigation with a molecular model of some kind, and finishes with a more precise one. (a) In the determination of the structure of a molecule, he starts from a knowledge of the number and kinds of atom constituting the molecule, and uses the experimental results to fill in quantitative detail about the interatomic distances, angles, and forces. (b) Thermodynamic studies lead to refinement of the molecular model.¹ For instance, determinations of the strengths of acids give values for the energy and entropy changes in certain reactions, and so lead to a closer knowledge of the molecules concerned. (c) Studies of the rates of chemical change have thrown much light on their detailed mechanism; for instance, on the question whether in a reaction of the type $AB + C \rightarrow A + BC$ the atom or group B becomes attached to C before or after it becomes fully detached from A.

In such investigations we are not *testing* the molecular model. We take for granted a preliminary model, and seek to make it more precise. The refining of a model is done by the experimentalist, not by constructing theoretically a more refined model and testing its consequences. The improved model is not itself normally the hypothesis that guides the experimentalist; it is formulated only as a result of the experiments. Experiment does not wait upon theory; it supplies new information on its own account.

However, the experimentalist is guided by certain hypotheses; otherwise his work would not be systematic. But these hypotheses are much vaguer than the molecular models; they concern rather the choice of experiments, the programme of research. For instance (a) the choice of substances for molecular-structure determination might be determined by the hypothesis that there is some relation between the lengths of single, double, and triple bonds in molecules; the experiments lead to values of these lengths, and possibly to a relation between them. The initial hypothesis (which may or may not be confirmed) is not to be identified with the molecular models which result from the investigations. Again (b) experiments to

¹ In certain simple cases it is possible to deduce some of the thermodynamic properties of a system from the precise molecular model mentioned in (a), by applying statistical mechanics. For instance, the specific heats and entropies of gases, and the equilibrium constants of gas reactions, can be calculated if the molecules are not too complex. In such cases the model is confirmed by the agreement of its consequences with experiment. For most liquid or solid systems, however, such predictions are impossible at present. See for instance Fowler and Guggenheim, *Statistical Thermodynamics*, Cambridge, 1939.

determine the strengths of acids might be guided by a hypothesis about the electrostatic effects of various atoms, which would influence the choice of acid, and by a hypothesis about the solvation of ions, which would suggest that the temperature should be varied; these hypotheses would necessarily be much less specific than the precise numerical results on the strengths of the acids.

The term 'hypothetico-deductive' must therefore be used with care if it is not to be misleading. If the term 'hypothesis' is used broadly enough, so that it covers the guesses that inspire research programmes, as well as detailed molecular models, it can be said that all systematic scientific work is hypothetico-deductive. (The information derived from chance observations and from the empirical arts and crafts would need special treatment.) But it cannot then be said that the testing of hypotheses is the sole or even the typical aim of science in all its stages. This thesis can be admitted for certain phases of science, in which experiment does no more than confirm or falsify a hypothesis; Pasteur's experiments, for instance, simply falsified the hypothesis of spontaneous generation under the conditions used. The thesis may also be true when theory is unusually advanced relative to observation, as in cosmology. But there is a stage in science, represented by physical chemistry today, where the hypothesis that gives rise to a research is usually much less precise than the conclusions of the research. The hypothesis is not of a kind to predict the exact results of the experiments, only to set the programme. The experimental measurements do not merely test a hypothesis; they lead to a more exact molecular model. The hypothesis may be confirmed or falsified, but in either case a more precise picture has been obtained. Improvement, rather than testing, of the theoretical picture is the characteristic activity of experimental physical chemistry.

There is a further question, namely how far theories are deliberately tested. All hypotheses when they are first conceived must of course be 'tested' against the known facts; the question is how far it is usual for a theory to be experimentally tested by comparing some deduction from the theory with a *new* observation. Examples that come to mind of theories that have been so tested are the wave-theory of light (by the velocity of light in water) and the general theory of electromagnetic waves (by Hertz's experiments on radio waves). These were clear-cut, mathematically exact theories, so that precise deductions could be made from them, about observations that could be carried out with known techniques. These conditions are not always satisfied.

The atomic theory is a case in point. Dalton realised that the union of atoms would account for the laws of chemical combination, and was able to interpret most of the data on combining weights in this way. The theory accounted for some of the puzzling facts of chemistry. As it stood, there appeared to be no independent way of testing it; no new phenomena were predicted. It was late in the nineteenth century when phenomena attributable to single molecules were discovered, as in Crookes's spinthariscopes, where a fluorescent screen showed the impact of particles from radioactive substances. In Dalton's time such techniques for testing directly the view that matter is divisible into molecules did not exist. These would have provided tests of the theory, but by the time they were discovered it was taken for granted. It had proved its worth not by predicting new observations in advance, but by explaining *post facto* the puzzling facts which experimentalists discovered.¹ A contemporary example of the same difficulty occurs in reaction kinetics. The equation which is found empirically to describe the variation of the rate constant (k) of practically any reaction with temperature (T) is: $k = A \exp(-E/RT)$, where A and E depend on the reaction. This can be interpreted if we suppose that reaction occurs not at every collision between reactant molecules, but only when molecules collide with sufficient energy and appropriate orientation. But there is no direct way of testing this hypothesis, comparable with the phenomena attributable to single atoms or molecules.

There is another difficulty about testing theories, namely the well-known difficulty about crucial experiments. The refutation of a theory is not a simple matter. A theory can only be tested in a particular case, so that subsidiary hypotheses are necessarily concerned in the prediction of the result of the experiment. Then if the prediction turns out to be at variance with experiment, it is the whole conjunction of hypotheses that is falsified, and the question which of these hypotheses is incorrect remains to be settled.² For example, the kinetic theory of gases is a combination of the hypothesis that a gas consists of molecules in motion in a space much larger than themselves, with the hypothesis that Newtonian mechanics applies to such particles. Disagreement with experiment, if observed, would show that one or other of these

¹ The theory was modified and elaborated to take account of new facts, but that is not the point here. See J. C. Gregory, *A Short History of Atomism*, London, 1931, chap. 8.

² This point was raised by Duhem, and has been discussed by Braithwaite in *Scientific Explanation*, Cambridge, 1953.

hypotheses was false, but not which of them. Actually there is no discrepancy in this case. But application of the same two hypotheses in another case, namely spectroscopy, would lead to results at variance with experiment; agreement with observation is obtained, however, if we use quantum mechanics instead of classical. Quantum mechanics in turn failed when first applied to the prediction of the specific heat of hydrogen at low temperatures. However, the discrepancy disappeared when two varieties of hydrogen, ortho and para, were discovered; the quantum theory was retained but the details of the molecular picture were modified. From these examples it is plain that very general hypotheses such as those of Newtonian mechanics or the atomic theory will not be easy to refute, since agreement with experiment can often be restored by adjusting a subsidiary hypothesis, as in the last instance cited. If the general hypothesis is untrue, however, these *ad hoc* adjustments will ultimately become incompatible with one another or with new experimental evidence. But in such instances there will be no single crucial experiments.¹ Only when two hypotheses share a common theoretical background can a crucial experimental test between them be arranged.² This can often be done in chemistry, where we can usually take for granted the most general hypotheses about atoms, molecules and quanta, and confine ourselves to adjusting the molecular model. It is possible, for instance, to decide whether the catalysis of a given reaction by acids is due to hydrogen ions alone, or to undissociated acid molecules as well.³ But even with relatively specific hypotheses, a crucial test is not always easy to arrange.⁴

The upshot of this discussion on theories is that in chemistry theories do not appear to be invariably subjected to deliberate test, nor are observations always or even usually made in order to test theoretical

¹ An interesting example is the phlogiston theory. Lavoisier's experiments on the calcination of mercury are often supposed to have been decisive against the phlogiston theory, but the results are explicable by Cavendish's version, in which the gas evolved (oxygen) is supposed to be water minus phlogiston. Cf. Cavendish, *Phil. Trans. Roy. Soc.* 1784, 74, 119; Alembic Club Reprints, No. 3 (1893).

² Such an experiment tests not the rival hypotheses alone but the combination of each with the system of hypotheses constituting the background. It may be that the background is wrong, as in the case of spectra mentioned above. Cf. also Popper, *op. cit.* (b).

³ Cf. R. P. Bell, *Acid-base Catalysis*, Oxford, 1941

⁴ For an example, concerning Wöhler's classic synthesis of urea, see Frost and Pearson, *Kinetics and mechanism*, London and New York, 1953, pp. 257 seq.

models. There is a large class of experimental investigations whose aim is to improve the theoretical molecular models, rather than to test them; and correspondingly these models are subjected less to testing than to adaptation, elaboration, and exact specification. This is characteristic of physical chemistry, which is at a stage where it is dominated neither by empirical method nor by theoreticians, but where experimentalists contribute important material for the development of theory.

2 *The testing of theories by means of critical cases*

I turn now to the question whether, when theories are tested, they are tested by applying them to those special cases in which they give results different from other theories, and in which they would be expected to fail if they were not true. It is easy to give examples of theories in physics that have been so tested: the theory of relativity, by observations on the perihelion of Mercury; the wave theory of light as contrasted with the particle theory, by determination of the velocity of light in water; the theory of the æther, by the Michelson-Morley experiment; the kinetic theory of gases, by the independence of pressure of the viscosity and conductivity of a gas. These are all rather general theories, to which a single genuine exception would be fatal; hence the interest of the critical observation. With less general hypotheses, however, falsification in a particular instance may merely reflect the limits of applicability of the theory. Moreover the tests of the above theories involve a minimum of subsidiary hypotheses; with more complex theories, such clear-cut tests may be more difficult. Let us see how these comments apply to chemistry.

Theoretical hypotheses in chemistry are of various degrees of generality and complexity, and they are tested in different ways, as follows.

(a) Some theories apply to all kinds of matter in all states; for example quantum mechanics, and the theory that matter consists of molecules, atoms, and subatomic particles. A single genuine instance of falsification would render these theories suspect. But, as we have seen, falsification of a general theory is difficult, since subsidiary hypotheses will be jettisoned first; in practice a theory will only be abandoned if a rival theory shows itself superior in some way. Moreover, the old theory may turn out to be a particular case of the newer, as classical mechanics is a limiting case of quantum mechanics.

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(b) Some bodies of theory—thermodynamics and statistical mechanics—apply in their simpler forms to all systems in equilibrium. In general their predictions are verified; the few cases where this is not so can be attributed to deviations from equilibrium.¹ The general theory would again be difficult to test.²

(c) More specific theories are applied to the several states of matter. Gases are treated for some purposes by the kinetic theory, for others by quantum-statistical theory. Liquids and solids are treated by applying statistical mechanics to various models, such as the lattice or the cell model for liquids, or Debye's coupled-oscillator model for solids. Solutions are treated by methods analogous to those for liquids, with the addition for electrolyte solutions of the theory of ions and their interactions. These theories allow the deduction of the general form of relations that can be studied by experiment, such as Debye's equation relating the specific heat of a solid to the temperature, or Arrhenius's equation relating the rate of a reaction to the temperature.³ To this extent the models can be tested; Debye's equation, for instance, holds quite well except at low temperatures. However, the theories do not generally predict the numerical values of the parameters in the equations; these are determined by experiment.⁴ To this extent the observations are not concerned only to test the model, but rather to make it more precise.⁵ If the model becomes established, observations are not used to test it at all, but only to give quantitative detail to the particular case. Arrhenius's equation, for example, has been confirmed so often that it is nearly always the values of the parameters that are of interest, rather than the form of the relation.

(d) Specific theories about molecular structure apply to individual substances. The molecular constitution of a compound can generally be settled on chemical grounds; at this stage hypotheses are tested,

¹ For instance, the apparent 'critical point' of an imperfect gas is in conflict with the Phase Rule which expresses the thermodynamic conditions of equilibrium. The anomaly arises because there are fluctuations in the system, which thus cannot be treated as in equilibrium.

² In retrospect it can be seen that classical statistical theory might have been questioned because it gave a wrong value for the entropy of a gas, which is given correctly by quantum statistics; but the discrepancy was not regarded in that way at the time, and it seems to have played little part in the displacement of classical by quantum theory.

³ See p. 215

⁴ See p. 214

⁵ These parameters could in principle be predicted by an adequate theory of intermolecular forces, but this has so far been forthcoming only for electrolytes in solution, where the forces are unusual in their magnitude and long range.

for instance the hypothesis that carbon dioxide contains the molecule CO_2 . The results of spectroscopic and diffraction experiments lead to conclusions about the spatial arrangement and dimensions and motions of atoms within the molecule. These data are not tests of the molecular theory, but give the model more precise specification. From them can be calculated, with the aid of statistical mechanics, such thermodynamic quantities as specific heats and equilibrium constants. These are generally confirmed by experiment; if they are not, the model is modified.¹ In this case, the details of the model are tested, but not its general applicability.

(e) Specific hypotheses about the detailed mechanisms of individual reactions may be supported by measurements of reaction rates and other data. Here it is often possible to test hypotheses and choose between alternatives, and a considerable body of theory has been built up for the reactions of organic compounds. Once this has been done, however, the interest shifts to the quantitative specification of the details of the model, as in (d).

From this brief survey of the theoretical hypotheses used in chemistry, it appears that, though chemists may sometimes test theoretical models in critical cases, this is not characteristic of chemistry. The most general theories (types (a) and (b)) would be difficult to refute in this way, and are taken for granted; only a whole series of anomalies would lead us to doubt them. Theories of restricted application (type (c)) are tested, in their earlier stages at least, but their protagonists will begin by testing them in cases where confirmation is expected rather than in critical cases, and will seek to extend the range of phenomena to which the theory can be successfully applied. For instance, the Debye-Hückel theory of electrolyte solutions was first tested for very dilute solutions, to which it might be expected to apply; it was not regarded as falsified by its inability to account for the properties of strong solutions. Falsification in a particular case would normally be taken to mean only that a theory had been applied outside its proper sphere.² Deliberate attempts to refute a theory are generally made

¹ There are sometimes discrepancies between calculated and experimental values for gases; internal rotation in molecules is one reason for this.

² For instance, the Debye-Hückel theory of strong electrolyte solutions in water does not hold at concentrations as high as 0.1 molar, because its basic assumptions are not then fulfilled. Similarly van der Waal's equation of state for a gas fails in the critical region, since the assumption that the volume of the gas molecules is negligible compared with the total volume is no longer applicable.

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only if it is in some way unsatisfactory, or if an alternative theory seems likely to be more successful. Chemists are more interested in supporting some theory, and in considering the theory *within* its range of applicability, since here it acts as an explanation of otherwise puzzling facts. Specific hypotheses of types (d) and (e) are tested in their early stages; later attention is directed to filling in the quantitative detail of the molecular picture. These divergences from Popper's account are probably due to the prevalence in chemistry of more complex, and more specific, theories than he had in mind.

The emphasis in Popper's account of the testing of theories seems to be that of the theoretician, who is interested in inventing, testing, refining, and replacing theories. It is not that of the experimental physical chemist, who is engaged in making researches which lead to more refined models, and who in the course of this work makes curious observations which he would like to interpret. It should apply best to those parts of science where theories are general in scope, precisely formulated, clear-cut in application, and ahead of experiment in the sense that they can be developed independently of experiment and made to give exact predictions. Such fields may be sought in mathematical physics. In chemistry, however, many of our theoretical hypotheses are by contrast restricted in scope, contain undetermined parameters, require additional hypotheses in application, and do not give numerically exact predictions. Consequently experiment and theory are more intricately linked. Physico-chemical measurements do not merely test hypotheses; they yield quantitative data about molecular models, which are thus refined by the efforts of the experimentalist rather than of the theoretician. This phase of science, which probably occupies most physical scientists today, has been curiously neglected.

The conclusions of this examination of contemporary chemistry may be summarised as follows. (i) Chemical research, inasmuch as it is systematic, uses hypothetico-deductive methods, in the sense that experiments are designed in the light of hypotheses. (ii) The refutation or support of these hypotheses is not however the main purpose or result of experimental investigations in physical chemistry; the experiments, besides testing an imprecise hypothesis, usually give new and precise information, which leads to more refined molecular models. These models are not commonly tested; the rôle of experiment is usually to give them more exact specification. The improvement of theories is a characteristic result of such work. (iii) Chemical theories

are not as a rule deliberately subjected to specially critical tests. These conclusions support the thesis that the method of science is hypothetico-deductive, but in a modified sense which recognises that the testing of hypotheses is not necessarily the central activity in a developed science; while the thesis that theories are tested by attempts to falsify them is not supported.

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NOTES AND COMMENTS

Psychological Concepts and Linguistic Restraints

It is often observed that an important part of the meaning conveyed by words and phrases derives from the context in which they are commonly used. Not infrequently the meaning is associated with imagery of particular types of events as in the phrases, 'These experiences made a *deep and lasting impression upon him*', and 'His arrival *interrupted the flow of discourse*'. It may be expected therefore, that difficulties will arise when words are used in special contexts, different from those with which the word is usually associated.

The words in everyday use have developed in association with the needs and adjustments of human beings who are assumed to be conscious and capable of purposive behaviour. Verbs such as 'strive', 'search', 'hope', 'pursue', 'succeed', 'follow', and 'continue' are quite consistently used in describing human behaviour and so too, nouns such as 'sorrow', 'sadness', 'remorse', 'regret', 'expectation' and 'anticipation', or any other derivative parts of speech. In the literature of psychology, however, certain specialised assumptions are sometimes made where there is a conflict between the normal usage or meaning and the assumptions. These include:

(1) *Difficulties Associated with Anthropomorphism.* Following the strictures of the late Professor Lloyd Morgan, psychologists and biologists generally have been aware of the fallacy of anthropomorphism, i.e. attributing to animal organisms abilities and capacities of intention and experience which may be in excess of those actually possessed. Yet in the description of the behaviour of lower organisms, as in the classic description of *Amoeba proteus* by Jennings,¹ phrases and words such as 'the cyst was held so that . . .', 'The Amoeba continued to follow . . .', 'did not succeed', and 'chase', occur; and not unnaturally, for if one tries to write a description of continuous behaviour but on the understanding that no qualities of purpose and foresight are to be implied, a genuine dearth of suitable words is encountered.

(2) *Linguistic Difficulties of Behaviourism.* Similar problems arise if the description of human behaviour is to be in conformity with some behaviouristic programmes. J. B. Watson, for example, eschews, 'Psychology as a science of consciousness. . .'.² The reader will find no discussion of consciousness and no reference to such terms as sensation, image, will and the

¹ H. S. Jennings, *The Behaviour of Lower Organisms*, New York, 1931—first published 1906. Present ref. p. 13

² J. B. Watson, *Psychology from the Standpoint of a Behaviourist*, Philadelphia and London, 1929, p. 2

like. These terms are in good repute, but I have found that I can get along without them.'¹

It is conceivable that by observation of human behaviour 'out there', a number of relatively consistent conjunctions could be established; but if Watson's programme is to be implemented, words which by implication import considerations of consciousness are clearly inconsistent with his intentions. Words such as 'try', 'persevere', 'endeavour', 'hope', 'elation', 'disappointment', 'sorrow', 'sadness', or any of their derivatives are clearly out of place and so too, phrases like 'in order to', 'so that', 'lest', and many others, strictures which in fact Watson did not observe. A completely empirical science at the molar level which excludes reference to consciousness might be devised by defining such states as sorrow, disappointment and sadness in terms of movements, gestures, and facial expression; but unless at some point, reference was made to introspective data and the previous usage of such words in literature, the definitions or designations would tend to be arbitrary.

A related problem is encountered by the plan of the late Professor Clark L. Hull who claimed that, 'The present approach does not deny the molar reality of purposive acts (as opposed to movement), of intelligence, of insight, of goals, of intents, of striving or of value; on the contrary, we insist upon the genuineness of these forms of behaviour. We hope ultimately to show the logical right to the use of such concepts by deducing them as secondary principles from the more elementary primary principles.'² It is difficult to see how these deductions are to be made unless at some stage, statements of the invariable conjunction of outwardly observable or behaviouristic designations with intentions and purposes are made; that is, the conjunction of behaviouristic designations with words indicative of intentions and purposes. Very few, if any, invariable conjunctions of particular movements with particular intentions could be made, however, even within one culture; but assuming that Hull had pursued this impossible programme, the same obligation to avoid importing implications of purposive activity in words used in behaviourist designations would occur in the initial stages, and at some stage the behaviourist would need to have recourse to introspective evidence and the words commonly used to refer to it.

Tolman's approach to behaviourism which assumes that 'Behaviour as behaviour, that is as Molar, is purposive and is cognitive'³ . . . (and) . . . as a matter of first identification, behaviour as behaviour reeks of purpose and of cognition,'⁴ would enable him in principle to avoid many of the linguistic conflicts of other behaviourists; but it should be remembered that the

¹ Watson, *op. cit.*, p. xii

² C. L. Hull, *Principles of Behaviour*, New York, 1943, pp. 25-26

³ E. C. Tolman, *Purposive Behaviour in Animals and Men*, London and New York, 1932, p. 12

⁴ *Ibid.*

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'pragmatically conceived objective variables' in the environment, such as 'discriminanda', 'discriminanda expectations', 'means-end-relationships', and 'sign-gestalt-expectation' are arrived at by observation of repeated trials by rats in mazes or puzzle-boxes¹ with the ever-present danger of anthropomorphic error.

(3) '*Production*' in the *Causal Relationship*. The normal usage of many verbs does imply that the effect is 'produced' by the cause. Sentences such as the following are often noted, 'Such a policy only leads to laxity', 'The exercise of authority at such an early age made him intolerant', 'Sustained effort will be necessary to bring about a change', 'He is quite obdurate as a result of his earlier disappointments', 'The solutions must be warm to secure the best results', 'His lectures send me to sleep', 'That grin of his always puts me off', 'The holiday is doing him good'. If, however, the empirical or regularity view of the causal relationship is adopted, the familiar problem of conflict between basic assumptions or viewpoint and implications deriving from the normal use of words is encountered.

Hume's statement of the empirical position was, 'We have no other notion of cause and effect, but that of certain objects, which have been *always conjoined* together, and which in all past instances have been found inseparable. We cannot penetrate into the reason of the conjunction. We only observe the thing itself, and always find that from constant conjunction the objects acquire an union in the imagination'.² If this point of view is consistently applied there would appear to be few, if any alternatives to the phrase 'invariably associated' when the notion of causal relationship is to be presented. However, the empirical view of the causal relationship is only attained after considerable reflection and it is inevitable that the everyday usage of words with their associated imagery should reflect the unsophisticated view that the effect is a necessary consequence of the cause or that in some way, the cause 'produces' the effect.

There is, however, one interpretation of the word 'cause', noted by Collingwood,³ where the suggestion of 'production' is less discordant with the empirical view than in the above examples. The implication is that an attitude in a conscious person is attained in response to the persistent efforts of another, as indicated in the sentences, 'He eventually brought me around to his point of view', 'He induced me to accompany him', 'He persuaded me to accept'. It could be claimed that all that is presented in the foregoing statements is the temporal priority of persuasion or inducement to acceptance; but the image suggested by these statements is that of the persistent effort of persuasion contending with conscious and possibly reasoned resistance and for

¹ See, *Collected Papers in Psychology*, California, 1951

² D. Hume, *A Treatise of Human Nature*, London, 1874, Part III, Sup. VI, p. 394

³ R. G. Collingwood, 'On the So-called Idea of Causation', *Proc. Arist. Soc.*, 1937-38, 38, 85-112

this reason, the suggestion that the persuasion 'produces' the acceptance may involve less of a challenge than in some of the cases discussed above. It would nevertheless be impossible to indicate any perceptible connection between the last phase in the process of persuasion and the acceptance or to insert anything between them and in this sense, penetrate the conjunction.

4. *Statements Relation to Emotions.* Many statements relating to emotional conditions indirectly present or imply a theory of the relationship of an emotional state to behaviour. Statements such as, 'He shook with anger', 'I was too scared to move', or 'speechless with terror' or 'paralysed with fright', or 'He was so elated that he found it impossible to work', suggest that the emotional state is a cause of the outwardly observable behaviour; but the form of the emotion is not specified either as a physiological disturbance or as a state of feeling. In statements such as, 'I felt so disgusted that I got up and left', or 'I was so overcome by a feeling of unworthiness that I left as quietly as I could', a state of feeling has by implication, causal status.

The literature on emotions and in particular the relationship of emotional states to behaviour is, however, not conclusive on a number of basic issues. The implication of the above sentences that behaviour is a manifestation or the state of feeling and an effect of it would be in accord with McDougall's view¹ but not with the James-Lange theory² in which the sequence would be that we see the angry bull, run, and are afraid. Emotional states in this theory are regarded as feelings associated with bodily changes elicited by the external environment. Once again, normal linguistic usage tends to be consistent with a particular theoretical position; but in view of the many aspects of emotional activity, it is difficult to account for the degree of consistency in referring to emotional states which has been achieved in everyday language.

For the more violent emotions, such as anger, fear, and sexual excitement, all three indicia, feelings, physiological variations, such as those of pulse rate and blood pressure, and outwardly observable behaviour, such as involuntary and voluntary movements, are assumed to be manifestations of the same emotional state. Indeed some of the movements may even acquire a certain currency such as the shaking of the fist to indicate anger, grimacing to indicate nausea, and rolling of the eyes for ecstasy. Mime and ballet or the unvoiced portrayal of emotional states can be understood and have, in fact, an appeal over a wide range of cultures. In ancient Egypt, the signs portraying anger derived from the picture of a charging bull or an angry baboon, suggesting that some association between introspective data and outwardly observable behaviour had been appreciated in man and was applied to animals. The consistency with which physiological variations, states of

¹ Wm. McDougall, *An Introduction to Social Psychology*, London, 1923, p. 462

² N. L. Munn, *Psychological Development*, Boston, 1938—see for a good discussion.

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feeling, and observable behaviour are associated is all the more remarkable when it is reflected that a given situation may not be appraised by different persons in the same way even within one culture and that there may be differences in intensity of reaction in addition to qualitative differences. With adults, observable indicia of emotional imbalance are often successfully repressed. Again, in terms of much physiological data, it would be difficult to distinguish between fear and anger. In many animal species fear is not always associated with flight. With human beings, there is the much quoted case of 'flight towards the enemy', on the eastern front in World War I.

The fact that physiological variations or states of feeling or observable behaviour may be used to indicate emotions may be associated with a certain laxity in referring to them; but it is interesting that unreflecting linguistic usage does support the view that states of feeling are the causes of and even 'produce' behaviour. The James-Lange theory is not without some experimental support;¹ but everyday linguistic usage presents by implication a different view.

The foregoing are but a few examples of difficulties of a kind which are likely to be encountered by those who construct special systems in psychology. The connotation of words in common use is often consistent with a long current system of thought and if these same words are used for any special purpose, conflicts between the connotation consistent with established usage and the demands of the new system are likely to arise.

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¹ See p. 226, n. 2

DISCUSSIONS

THE FALSIFIABILITY OF THE LORENTZ-FITZGERALD CONTRACTION HYPOTHESIS

PROFESSOR A. Grünbaum's 'correction' of Professor K. R. Popper's assertion of the non-falsifiability of the Lorentz-FitzGerald contraction hypothesis¹, though it has been accepted by the latter, is nevertheless much more misleading than the original statement. The hypothesis was certainly *ad hoc*, for the *realisation* of its falsifiability came very much later, after it had been displaced by the special relativity theory of Einstein. FitzGerald apparently did not publish his suggestion (there is an account of its genesis in *The Ether of Space*, by O. J. Lodge (New York, 1909, p. 65)), but Lorentz's independent account was first given in 1892.² He restated the hypothesis three years later, in a paper which has been translated into English (in *The Theory of Relativity*, by Einstein and others, London, 1923) and will serve as the most suitable source of reference.

In this paper, Lorentz, after describing the Michelson-Morley experiment, says: 'If we assume the arm which lies in the direction of the Earth's motion to be shorter than the other by $\frac{1}{2}Lv^2/c^2$, and, at the same time, that the translation has the influence which Fresnel's theory allows it, then the result of the Michelson experiment is explained completely. . . . Surprising as this hypothesis may appear at first sight, yet we shall have to admit that it is by no means far-fetched, as soon as we assume that molecular forces are also transmitted through the ether, like the electric and magnetic forces of which we are able at the present time to make this assertion definitely. . . . It is worth noticing that we are led to just the same changes of dimensions as have been presumed above if we, *firstly*, without taking molecular movement into consideration, assume that in a solid body left to itself the forces, attractions or repulsions, acting upon any molecule maintain one another in equilibrium, and, *secondly*,—though to be sure, there is no reason for doing so—if we apply to these molecular forces the law which in another place we deduced for electrostatic actions.'

It is clear from this, first, that Lorentz had no idea at all at this time of any 'time dilation'; and secondly, that he assumed ('though to be sure, there is no reason for doing so') that uncharged bodies would behave as though they were charged. There could scarcely be a clearer case of an *ad hoc* hypothesis. With regard to the possibility of falsifying it, Lorentz wrote: 'One could hardly hope for success in trying to perceive such small quantities except by means of an interference method. We should have to operate with two perpendicular rods, and with two mutually interfering pencils of light, allowing the one to travel to and fro along the first rod, and the other along the second rod. But in this way we should come back once more to the Michelson experiment, and revolving the apparatus we should perceive no displacement of the fringes.' So the only possibility of falsifying the hypothesis that he could imagine was a repetition of the experiment that had generated it.

¹ This *Journal*, 1959, 10, 48

² Lorentz, *Arch Néerl.*, 1892, xxv, 363

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Of more topical interest, however, is the present state of the question. Lorentz first deduced his transformation in its complete form in 1903.¹ This was based on (1) his extension of Maxwell's electromagnetic theory to cover moving bodies, involving the assumption of a fixed ether and (2) what was then known as 'the electric theory of matter'. The 'contraction' then ceased to be an *ad hoc* hypothesis, and became falsifiable by anything that would falsify these two assumptions. Shortly afterwards, Einstein² introduced his special theory of relativity, in which a fixed ether was rejected, but the deduction from the fixed ether theory that 'light is always propagated in empty space with a definite velocity c which is independent of the state of motion of the emitting body' was retained as a 'postulate'. He could then dispense with the assumption of the electrical theory of matter and show that the Lorentz transformation would hold for all bodies, charged or not. Einstein's theory, as he clearly recognised and stated, rests on two assumptions—the assumption of no ether (i.e. no standard by which one state of motion of a single body could be distinguished physically from another) and the assumption that the velocity of light in space is independent of the state of motion of the emitting body.

Both these assumptions are falsifiable, though in different senses. The first would be falsified if a physical effect were found that would distinguish which of two relatively moving bodies could more properly be called the moving one, and it can never be proved that no such effect is possible. The second assumption, however, could be made the subject of a laboratory experiment to compare the times of arrival of pulses of light, proceeding from bodies in relative motion but emitting at the same point, at a distant point along the line of motion. This was, of course, impracticable in 1905, but it should not be beyond the power of modern techniques. It is highly desirable to test, if possible, this basic assumption of existing physical theory.

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THE PARADOXES OF CONFIRMATION—A REPLY TO DR AGASSI

IN his note in this journal (1959, 36, 311-317) Dr J. Agassi criticises an earlier note of mine (this *Journal*, 1958, 35, 227-233) in which I discussed some of the problems associated with the paradoxes of confirmation. I feel that Agassi's comments, forceful though they are, do not invalidate the main points I was trying to make, and I should therefore like to attempt to clarify my position. It may be that such clarification will show Agassi that our respective views are not so completely incompatible as he seems to think.

I

Unfortunately in replying to Agassi it is necessary first to discuss a question of exegesis, since he accuses me in his note of misrepresenting Watkins by attributing to him views which he rejected 'by clear implication'. I shall therefore show in the first part of this note that this 'implication' was very far from clear. Such a discussion is bound to be somewhat tedious—I regret its necessity—but it may all the same serve the subsidiary function of providing a summary of the main issues involved.

¹ Lorentz, *Proc. Amst. Acad.* 1903, vi, 809

² Einstein, *Ann. d. Phys.*, 1905, 17, 891

If one considers the hypothesis 'All ravens are black', one is inclined to accept the following proposition (*a*): The hypothesis 'All ravens are black' is empirically backed by observing a black raven but not by observing a white swan. Yet the hypothesis is equivalent to 'All non-black objects are non-ravens' and in this formulation white swans would seem to provide possible backing. In a passage I quoted in my earlier note Watkins writes

On a Popperian theory of confirmation this hypothesis is confirmed by an observation-report of a black raven, not because this reports an instance of the hypothesis—a white swan is also an instance of it—but because it reports a satisfactory test of the hypothesis: a raven has been examined successfully for non-blackness. Statements about non-ravens which do not report tests of our hypothesis cannot confirm it.

I interpreted this passage as implying the acceptance of proposition (*a*); Agassi suggests that it 'clearly implies' its rejection.

Now (*a*) is the conjunction of two statements (*a*₁) 'The hypothesis is empirically backed by observing a black raven', (*a*₂) 'The hypothesis is not empirically backed by observing a white swan (or a white shoe)'. It is, I think, obvious that the first sentence of the passage above clearly implies the acceptance of (*a*₁). Whether the last sentence implies the acceptance or rejection of (*a*₂) depends on how it is read. If it is interpreted, as I interpreted it, as if it were 'Statements about non-ravens—which do not report tests of our hypothesis—cannot confirm it', it implies acceptance of (*a*₂); if read as 'Statements about non-ravens that do not report tests . . .' it implies the rejection. I see now that in my interpretation I may have been misled by the context, but I would suggest that the implication is *not* entirely clear.

Agassi also cites a passage in an earlier article by Watkins. To quote Agassi, 'Yet, Watkins adds, if we merely ask the man to tell us about the black ravens he has seen or heard of, his report "I've seen hundreds of thousands of black ravens" will *not* count as backing the hypothesis. (Thus again, by implication Watkins definitely rejects (*a*).)' But here again Agassi over-simplifies. For, in the passage to which he refers, Watkins actually writes *not* that black ravens 'will not count as backing the hypothesis' (Agassi's paraphrase) but that they will 'carry very little weight so long as question (*b*) [whether any non-black ravens have been observed] remains unanswered'. These phrases can hardly be said to mean the same. And even if they did, the most we would have would be Watkins implying the rejection of (*a*₁), the very statement which in the passage I quoted he accepts. What Agassi does is to take one passage which implies acceptance of (*a*₁) and, perhaps, rejection of (*a*₂), take another passage which could conceivably be interpreted as implying the rejection of (*a*₁), and then claim that there are *two different passages* in which Watkins implies the rejection of (*a*) which is the conjunction of (*a*₁) and (*a*₂). Logically this is valid, but in this context it is somewhat disingenuous. Instead I would suggest that it is by no means clear whether Watkins at the time he wrote these articles did or did not reject (*a*). I suspect that he tended to assume that observation of black ravens would always count as tests of the hypothesis, but that few, if any, observations of non-ravens would constitute tests.

We have thus by means of this exegetical inquiry reached what I consider the most important point. In his original article Watkins claims that if one uses the

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notion of a test of a hypothesis then one can avoid Hempel's paradoxes of confirmation. In my earlier note I suggested that this claim could only be justified if one clarified the notion of a test of a hypothesis. And this Agassi, like Watkins, fails to do.

For in the case of a zero-level hypothesis such as 'All ravens are black', what is to count as a test? One possibility is to interpret the concept as referring solely to the expectations of the observer. As long as the observer is thinking about the possibility of encountering a non-black raven any observation is to count as a test. But surely this interpretation would make the notion too subjective; anything would count as a test so long as the observer was in the right frame of mind, and what would count as a test for A would not count as a test for his companion B.

A more plausible interpretation would be to interpret the notion of test as having some reference to our prior knowledge about the (objective) probability of finding a non-black raven. This is the sense which seems to be implicit in Agassi's note when he points out that observation of a white shoe or a black raven will only count as a test 'if we have good reasons to suspect that we might find a non-black raven' (Agassi, 312). That is to say, for an observation to count as a test we must already know that, if there are any non-black ravens, there are few if any places where we are more likely to find them than here.

So on this interpretation we must have prior knowledge about ravens. But this is precisely what is ruled out by Hempel when he writes (*Mind*, 1945, 54, 20) 'If we are careful to avoid this tacit reference to additional knowledge (which entirely changes the character of the problem). . . .' That is, the paradoxes of confirmation are discussed by Hempel in those cases in which we consider the bearing of evidence E on hypothesis H *in the absence of all prior knowledge*. My point is that if we accept this proviso, then either the notion of a test of a hypothesis becomes inapplicable or else every observation will have to count as a test in so far as it is *logically possible* that anything we observe might have been a non-black raven. To express the situation in sporting terms, if Watkins decides to challenge Hempel on his own ground, then he ought to agree to play according to Hempel's rules. But if he does this then the paradoxes cannot be avoided.

The obvious answer is to refuse to play on Hempel's ground, just because it is far too restricted. For, as I pointed out in my earlier note, we never in practice find ourselves in the sort of situation which Hempel discusses—the sort of situation in which we know how to use the concepts 'black', 'raven', etc., but have no other knowledge about the probabilities of finding ravens or black objects. So Watkins and Agassi could with some justification claim that in any actual situation we do have enough prior knowledge to enable us to regard *some but not all* observations as tests of the hypothesis. If one does make this claim then the second half of my earlier note in which I tried to explain why we regard black ravens as better evidence for the hypothesis than white shoes, could be regarded as an attempt to discover the kind of prior knowledge we need for us to be able to apply the notion of a test of the hypothesis.

To sum up, Watkins in his two articles suggests that the reason why Hempel has to accept the fantastically counter-intuitive conclusion that white shoes, black shoes, etc., all confirm 'All ravens are black' is that he accepts an instantial theory of confirmation instead of the Popperian view that a theory can only be corroborated by withstanding severe tests. I have tried to show that there is a more fundamental

reason—Hempel's proviso that we must consider the case in which we have no prior knowledge. So long as one accepts this proviso, one will be unable to avoid the counter-intuitive consequences.

I have also suggested that Watkins and Agassi might answer this by rejecting the proviso, claiming that such a situation is so artificially simplified as to be of no relevance to the logic of scientific procedure. The difficulty here is that one is at first very tempted to follow Hempel who, I think, assumes that in any attempt at a rational reconstruction of our scientific knowledge one must finally get down to particular observation reports. If this is so, then the first step would presumably be to proceed to zero-level generalisations—that is generalisations containing observation-terms only and based *solely* on particular observation reports—and this is the type of case which he discusses in his articles. In fact I would agree with Popper (*The Logic of Scientific Discovery*, pp. 100 ff.) that any attempt to reconstruct our knowledge on a foundation of incorrigible basic statements is mistaken. What I wish to point out is that if one does reject Hempel's discussion of confirmation as artificial, one has to discuss this wider issue.

3

In the second section of his note, Agassi criticises the part of my note in which I tried to explain the origin of the paradoxes. I must confess that I find his criticisms very difficult to follow and can only conclude that I cannot have made my argument sufficiently clear.

In my note I confined my discussion to zero-level generalisations of the form 'All ϕ objects are ψ ' where ϕ and ψ are observational predicates. What I tried to discover was the conditions under which one could assume that observation of a group of ϕ objects all of which were ψ would count as more confirmatory than an equal group of $\sim \psi$ objects all of which were $\sim \phi$. The conclusion I reached was that this assumption would be justified if and only if the probability of an object being ϕ were less than the probability of an object being $\sim \psi$, or, in a finite universe, if and only if the number of ϕ objects were less than the number of $\sim \psi$ objects.

As far as I understand him, Agassi assumes that in the case of an observed object, e.g. a white shoe, what matters is not, as I suggested, the probability of this object being a non-black object (a $\sim \psi$ object) but the probability or improbability of its being a white shoe. That is, he seems to take me as suggesting that a *sufficient* condition for one piece of evidence to be more confirmatory than another is that it should be less probable. But this is, of course, ridiculous. If my pocket book on birds is correct, the probability of finding a black raven of length 28 inches and tarsus length 3.2 inches is much less than that of finding one of length 26 inches and tarsus 2.8 inches; but it would be stupid to claim that the former provided better confirmation of the hypothesis that all ravens are black. So in so far as Agassi's arguments are aimed against the view that improbability of evidence is a sufficient condition of strong confirmatory power, they are unnecessary; that was not the view that I was putting forward.

In general Agassi seems to assume that I was trying to solve the general problem of defining the concept of degree of confirmation of a hypothesis—and points out quite rightly that, if that were so, my solution would be inadequate. In fact, I was discussing the very limited—one might say almost artificially limited—problem of

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zero-level generalisations. And within these limits I was not even trying to establish any general concept of degree of confirmation but only attempting to state the conditions under which one group of instances could be said to provide *more confirmation* than an equal group of different instances.

So there were two respects in which my problem was more restricted than that discussed by Popper in the articles to which Agassi refers. Because of this I was able to discuss the simpler problem without bringing in any of the wider and more controversial issues which Popper had to deal with in his attempt to arrive at a concept of confirmation which would be adequate for the general case.

So I disagree with Agassi when he suggests that Popper's articles render any independent discussion of the limited problem completely otiose. What in any case is clear is that Agassi is wrong in claiming that Popper's analysis shows my condition to be necessary but not sufficient. For in these articles (reprinted as Appendix IX of *The Logic of Scientific Discovery*) Popper suggests that, as a first approximation, the support given by x to y could be defined by the function

$$E(x, y) = \frac{P(y, x) - P(y)}{P(y, x) + P(y)}$$

where $P(y, x)$ is the probability of y given x , and $P(y)$ is the absolute probability of y .¹ Now in my example one could write $P(y')$ as the absolute probability of finding that in a group of $n\phi$ objects none are $\sim\psi$, and $P(y'')$ as the probability of finding that in a group of $n\sim\psi$ objects none are ϕ . Both $P(y', x)$ and $P(y'', x)$ will be 1 since the hypothesis that all ϕ objects are ψ excludes the possibility of finding an object which is ϕ and $\sim\psi$. So

$$E(x, y') = \frac{1 - P(y')}{1 + P(y')}$$

and similarly for y'' . Thus $E(x, y') > E(x, y'')$ if and only if $P(y') < P(y'')$. And, as I argued in my note $P(y') < P(y'')$ if and only if the number of ϕ objects (or probability of an object being ϕ) is less than the number of $\sim\psi$ objects (or probability of an object being $\sim\psi$).

So on Popper's first suggestion—which, he says, satisfies 'the most important of the *desiderata*'—my condition would be sufficient. I am not sure whether this will also be the case on Popper's final definition of degree of confirmation as measured by

$$C(x, y) = E(x, y)(1 + P(x)P(x, y))$$

since I am uncertain as to the way in which one could assign values to $P(x)$ and $P(x, y)$.

4

In the concluding section of his paper, Agassi rejects my suggested interpretation of Popper's theory of corroboration as higher level induction. But in spite of Agassi's eloquent and interesting discussion and in spite of the much fuller argument by Popper himself in *The Logic of Scientific Discovery*, I find it difficult to see that the distinction between Popper's theory and inductivist theories is always quite as sharp as they claim.

For suppose one considers the relatively simple case in which the scientist is establishing what Popper calls a low-level empirical hypothesis, that is a hypothesis

¹ K. R. Popper, *Logic of Scientific Discovery*, London, 1959, p. 400

about a reproducible effect as opposed to a singular statement about the result of one particular experiment done at some particular place and time (op. cit., pp. 86-87). On the Popperian theory, the scientist presumably does the experiment once, formulates the hypothesis, performs the experiment once or twice more in order to test the hypothesis, and then decides that there is, at present, no point in carrying on with any more tests of the same kind. That is, even on Popper's theory, there is bound to come a time when the scientist decides that further tests are, for the moment, pointless. One cannot help asking whether this account is very different from the alternative account which speaks of the scientist reaching the conclusion that since the hypothesis has been confirmed several times it is probably correct.

Another type of situation in which the difference between the accounts is not immediately obvious is that in which a scientist is convinced that there is a law governing the relationship of two physical variables and sets out to discover what form this relationship takes (e.g. Snell's Law). We may imagine that he makes four or five different observations and plots the observed results on a graph. He then formulates a hypothesis as to the law and makes a considerable number of further observations, all of which support the hypothesis (fail to falsify it). On the inductivist account, one would regard all these observations as instances which confirm the hypothesis. On Popper's theory of corroboration, one would, I assume, regard the later observations as tests of the hypothesis. But would the observations made before formulating the hypothesis count as tests? If so, the different accounts become very similar. If not, there is introduced a rather strange symmetry. [To take an extreme case, would one say that the observations made by Tycho Brahe were tests of the hypotheses which we now call Kepler's Laws?]

It is very possible that I have misunderstood Popper and Agassi, and that these examples in fact raise no real problems. The general point, however, is the same as that discussed above—the difficulty of assigning any clear meaning to the phrase 'test of a hypothesis'. If one interprets it as referring to the attitude of the individual experimenter, then one seems to be retreating into the type of 'psychologism' which Popper rejects at the very beginning of his book. If on the other hand one interprets it in terms of prior probabilities and probabilities relative to the evidence, then there appear to be cases, such as the ones I have mentioned, in which our knowledge of the prior probabilities is so fragmentary that the idea of a test becomes inapplicable.

H. G. ALEXANDER

GALLIE AND THE SCIENTIFIC TRADITION

I

W. B. GALLIE has argued¹ that any set of criteria proposed as criteria for 'scientific' can be shown to be not necessary, not sufficient, or vague, and that 'scientific' can be explicated only by the ostensive means of pointing to the historical tradition of science. He has suggested positively that we should work to indicate and describe this historical tradition. Gallie's thesis is not novel, except perhaps in its generality.²

¹ W. B. Gallie, 'What Makes a Subject Scientific?', this *Journal*, 1957, 8, 118-139

² See, e.g. Friedrich Waismann, 'Verifiability', in A. Flew, *Essays on Logic and Language*, Oxford, 1951, and Max Black, 'The Definition of Scientific Method', *Problems of Analysis, Philosophical Essays*, London, 1954, p. 13

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It is precisely at this level of generality, however, that the thesis becomes interesting. I shall argue that Gallie's thesis, while basically sound and valuably suggestive, must be reformulated. Positively I shall indicate what I believe is a more tenable and fruitful arrangement of his ideas.

In the first place there seems to be a conflict between Gallie's programme for historical research and his conclusion that 'science' cannot be defined. Suppose a scholar needs to argue that a piece of his research is good; would he not in this case need a set of canons defining scientific procedure? Is there not, then, a sense in which he needs a definition of 'science' apart from an ostensive one? Nor would such a definition be valid merely for an historian (and not, say, for a physicist), because by hypothesis the programme we have in mind is to issue in results relevant to and valid for workers in all the sciences and related fields. It might be true that the historian in this case could justify his canons only by pointing to the scientific tradition, but the issue here concerns the *existence* of canons and not their justification.

In the second place Gallie does not push his scepticism far enough. I shall develop this point at some length, because it is the basis for my positive suggestions. Gallie takes for granted that science is objective, in that no observation or proof is acceptable to one scientist unless it is also acceptable to every other scientist; yet, if we examine this objectivity, we find that it too is irreducibly traditional. 'Any other scientist' means 'any normal, competent, unbiased person', but what are to count as tolerable ranges of normality, competence, and bias are defined by traditional choices. A non-traditional stipulation, for example that the scientist must be normal, is likely to be circular (for example if 'normal' is taken to mean 'normal enough to agree with other scientists') or else arbitrary (for example if 'normal' is defined by particular specifications about vision, education, etc.).

We cannot explain the objectivity of science by saying merely that scientific results must be repeatable, because repeatability can be secured for only some aspects of the original situation and the only way we can describe these aspects is to say that they are the relevant ones, which is to say the scientific ones. Nor does Hempel's explication¹ help us here. Hempel requires that observation terms have conditions of application which are well determined for each user of the language and the same for all users, but the delimitation of the class of users is traditional and so is the choice of level of determinacy.

Pollock, in his criticism of Gallie,² seems to have missed this general point. It is too simple to say that every activity involves communicability, system, and tradition, and that hence these cannot be taken as *indicia* of science. It is too simple because there may be different types of verifiability, communicability, and systematisation, with each type generating its own activity and tradition and some types definable only ostensibly but at least in that way identifiable. Gallie, however, has not been clear on this.

Gallie's argument is essentially negative and seems to emphasise our inability to find the kind of definition we want. But a stronger, positive argument is possible: Tradition is *necessarily* taken account of in scientific activity, not simply as a matter of psychological fact but as a matter of correct procedure. Tradition enters the activity

¹ Carl G. Hempel, *Fundamentals of Concept Formation in Empirical Science*, Chicago, 1952, pp. 10, 22, and note 5

² Seton Pollock, 'What Makes a Subject Scientific?' this *Journal*, 1958, 9, 130-132

of confirming theories, and more generally it is necessary to the vindication of any of the scientist's conceptual tools. The reason for this is that the scientist wants the vindication to be objective; in particular he wants the vindicating data to be different from the data which originally inspired the construction of the theory or concept. Typically a theory is constructed from all the data available now and vindicated by data gathered later. Because it involves this time lag, the constructing-vindicating sequence qualifies as a tradition at least in the narrow sense of involving a communication process over a period of time. The longer this period of time is, *ceteris paribus*, the greater will be the vindication; but, the longer the period of time, the more this internal communication process will be a tradition in the broad sense, involving not only communication and continuity but also change due to choices which are free and occasionally motivated by non-rational considerations.

Another part of Gallie's formulation which can be strengthened is the initial assumption about the kind of definition we want. Gallie's basic assumption is that we want 'science' to have as wide a denotation as reasonably possible, but this assumption will be challenged by anyone who wants 'science' to be an honorific term with a narrow denotation. Historians and sociologists of science will certainly prefer a wide denotation, and so will anyone who believes that inter-field co-operation can be enhanced by using a definition which unites the respective fields. The clearest and strongest position is simply to announce that one has in mind the interests of the historian and others, and that these interests are best served by conceiving science in a certain way. This latter statement, of course, must then be proved.

2

My own positive suggestion is, like Gallie's, motivated by the desire for a wide denotation. I should like 'science' to apply to the western scientific tradition deriving through the Babylonian, Egyptian, and Greek traditions; I should like it also to apply to the Chinese and pre-Columbian American traditions, to mathematics, and to the 'more scientific' parts of the metatheory of science. But for this purpose I believe it important to construe scientific activity in a new way. Gallie seems to conceive scientific activity as a single tradition containing many sciences, including for example psycho-analysis but not Christian Science, and demarcatable from the non-scientific by a relatively sharp line. I believe it better to regard scientific activity as a plurality of scientific traditions which are not sharply separate from non-scientific activity, which have certain concerns in common, and which are unitable in terms of these concerns. Historically there have been several traditions initially distinct but (with a few marginal exceptions) gradually merging. Psychologically there has been one tradition gradually fragmenting into compartments (cf. the 'tree of knowledge' metaphor). But epistemologically the many scientific traditions are derivative from a single background tradition, that of co-operative work. Science, I suggest, is the activity of discovering and summarising what must be agreed on in the context of communication associated with co-operative work.

Summaries include predictive theories, mathematical systems, and taxonomic schemes. A codification of grammar counts as such a summary if interpreted in a lexical rather than stipulative sense.

Discovering and summarising are activities which have vague boundaries. Theoretical scientists clearly are engaged in such activity. Anyone who discovers

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hard facts, that is, facts which command agreement, is doing science. This fact-gathering activity is often of a low-grade, nearly trivial character, but we can allow it as scientific because in each instance we can see clearly enough just how low- or high-grade it is and we can regard its products accordingly. What counts as high-grade data varies with the development of enquiry; at an advanced stage, for example, data may be high-grade relative to a theory which is being tested by them.

Must scientific work be done according to a certain schema? Gallie requires that a scientist be able ultimately to give an intelligible account of how he came to his conclusions,¹ but I believe it mistaken to insist on this, and I predict that scientists in the future will not insist on it. Aside from the vagueness of the notions of intelligibility and ultimacy there is good reason for not ruling out mere serendipity as an acceptable scientific procedure. Suppose we asked the guesser what his scheme of procedure was and he replied 'It just came to me'. If we then ruled his guess unscientific, why should we not also rule as unscientific all computations made by a mechanical computer? The subconscious mind of the guesser is like a mechanical tool he can employ in problem-solving, and there is no more reason why the guesser is any more obliged to say why his mind works as it does than the operator of the computer is to say why his machine gives the results it does. If the machine operator can give 'because the axioms and rules are as they are' as a reason, the guesser can give 'because my experience and brain matter are as they are'. Suppose the guesser is able to detail the logical relations of his guess to the rest of science; this ability would be irrelevant to the question of the scientific status of his guess, because it too might be the result of mere serendipity. The value of such detailing is that it helps others to see that the given item is indeed a fact or a good theory, and that the guesser is a man worth listening to in the future.

By 'co-operative work' I mean a certain tradition of human activity which has been described by historians of technology and by economic anthropologists. It is likely that arguments similar to those which prove that science involves tradition can also be used to show that co-operative work is a tradition in the broad sense. Co-operative work incidentally includes experimenting in laboratories.

Associated with such work is a type of communication which may be called 'rational communication', as distinct, say, from the communication appropriate to religious practice or solitary reverie. Rational communication has boundaries which can be made quite precise in practice. Members of the communication group propose to each other criteria for meaning, evidence, and inference, and they reject any criteria not agreed to by all members of the group. Since there is a definite purpose to be served here, namely, the enhancement of co-operative work, the criteria can be chosen with greater confidence and firmness than would be the case if the same communicants were trying to select criteria for 'communicability' or 'truth' in some much more general sense.

Because of the richness of the communication techniques used in direct connection with co-operative work there arise secondary, tertiary, etc., levels of discourse which are perhaps not directly connected with the original work but which belong to the communication context as long as it would be pointless to exclude them. Counting may have been used first in herding and mathematics has been elaborated at levels

¹ Gallie, *op. cit.*, p. 130

remote from herding, but mathematics still belongs to the field outlined by the original agreements.

Part of the communication associated with co-operative work might be regarded as extraneous to science, for example the singing which co-ordinates the paddling of a canoe. On the present analysis the song *qua* declarative statement might not compel assent and hence would not be scientific knowledge, but a report about the rhythm of the song might qualify as science.

'What must be agreed on' has the same initial vagueness and the same correctives for it as the gross structure of the communication context. The gross structure is defined by agreement on certain procedures. The fine content of the context is discovered by applying those procedures. The communicants must agree on what their procedures commit them to; the alternative is disruption of the communication context.

It is not the case in general that a summary must be agreed to. What must be agreed to are the items summarised, but for any given class of items there might be several schemes which are equally possible as summaries. On this view it makes no difference whether an empirical theory is regarded as more than a mere summary of the statements which are evidence for it, so long as the theory is *at least* such a summary. A mathematician is a scientist when he discovers or summarises implications between axioms and theorems. A psycho-analyst is scientific when he discovers facts (about patients) which compel the assent of not only the guild of analysts but anyone included in the wide rational communication context, and also when he summarises such facts.

Scientific activity on this view is continuous with other kinds of activity. To the extent that co-operative work is not sharply distinguished from other human activities science will have vague boundaries. Within the area of co-operative work the work of the experimental scientist will not be sharply distinguished from that of the superior artisan¹ and the work of the naturalist will not be sharply distinguished from the casual observations of any alert, sane adult. Near the other boundary of science the work of the theoretician will not be sharply distinguished from the work of the metaphysician or the seer who reports visions he has had. Why should we desire sharp boundaries? We can maintain high standards of scientific activity by acknowledging that scientific work can be high-grade or low-grade and formulating a criterion for high-grade scientific work. Such a criterion does not constitute another definition of 'science' but simply articulates an interest, namely, a direction we want scientific activity to take. Such interests are derived from the values of the culture at the given time. Today we might base our criterion for high-grade scientific activity on Kemeny's measure of the systematic power of a theory.²

Science has two types of cosmology. There is the primary cosmology of the human work situation, and there are secondary cosmologies formulated by the theoretical scientist. The latter range from interpretations of particular theories (as for example the expanding universe theory) to general interpretations of the totality

¹ Of the type studied by Edgar Zilsel, 'The Origins of Gilbert's Scientific Method', in P. P. Wiener and A. Noland, *Roots of Scientific Thought, A Cultural Perspective*, New York, 1957

² John G. Kemeny and Paul Oppenheim, 'Systematic Power', *Philosophy of Science*, 1955, 22, 27-33

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of all scientific theories, yielding for example a picture of 'nature as a whole'. We know much about secondary cosmologies and their traditional character. About the primary cosmology—about its content, history, and relations with science and scientific tradition—we know little; but no investigation of the scientific tradition can afford to ignore it.

DAVID HARRAH

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REPLY TO DAVID HARRAH'S DISCUSSION NOTE

MR HARRAH's note on my article makes some valuable points, e.g. that I simply take it for granted that all science aims at 'objectivity', that I omit to notice that 'tradition is necessarily taken account of in scientific activity . . . as a matter of correct procedure', and that I have written of *the* scientific tradition, whereas almost certainly there are several such traditions which are 'initially distinct . . . but gradually merging'. More important than these criticisms, however, is his own main suggestion that science is 'an activity of discovering and summarising what must be agreed on in the context of communication associated with co-operative work'.

This looks as though it might be made into an exciting philosophical hypothesis, and I have no wish to pooh-pooh it out of embryonic existence. But, aside from the inevitable sketchiness of Harrah's treatment, there are some very familiar objections which such a hypothesis is bound to evoke. What is the force of 'must be agreed on'? If it means simply 'must be agreed on *if there is to be science*' then the hypothesis loses most of its interest: it comes to little more than pointing out that the sciences have in point of fact arisen as social or inter-personal activities, and the questions of how and why they arose are still to answer. But perhaps Mr Harrah means, 'that must be agreed on *if communication is to succeed*'? But this will not do: think, e.g. of successful religious or successful aesthetic communication. What seems to me to be required is an explicit account of the kind of success which requires that certain standards and procedures shall be agreed on and that thus gives rise eventually to a scientific tradition. I hope that we shall hear more about this from Mr Harrah and others.

A minor point, which might, however, give rise to misunderstanding. Mr Harrah repeatedly affirms that, for me, 'the scientific' can only be defined 'ostensively'. But usually an ostensive definition is achieved through specimen cases which are indicated in a very special way, viz. to show that they are to be taken hereafter as standard cases of the concept to be defined. But, on my thesis, nothing like this can possibly be true with regard to science: and not simply because of the continual shifts within scientific tradition, but also because of the colossal diversity of the activities to be included under the term 'scientific'. My thesis was rather that a piece of work can be judged to be good or genuine science only by (at least implicit) reference to the main scientific tradition which lies behind it and which it represents even as it advances—and in advancing no doubt in a way alters—that tradition. Hence to demand the kind of definition that can be used to settle automatically the question of the value of a given piece of research is to my mind entirely illegitimate and misguided. My view implies that there can be no automatic tests of scientific value—

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unless in cases where we simply eliminate pieces of sham or intellectually despicable work, and in these cases the standards we apply are such as we might apply to almost any kind of intellectual endeavour. But when it is a question of deciding about relative *scientific* values, then our decision must rest upon judgment which must rest in turn, if it is to be justified, upon standards that have been *progressively revealed* in and through a relevant part of our scientific tradition.

W. B. GALLIE

MR WATKINS ON THE TWO THESES

IN his rejoinder¹ to my 'The Two Theses of Methodological Individualism',² Mr J. W. N. Watkins attempts to make two points. Of these, one has no bearing on the issue between us, whereas the other is entirely mistaken.

In my paper, I attempted to show that methodological individualists, in attempting to defend the thesis that all explanation in social science must be in terms of individuals or concepts exhaustively analysable into individuals or individual dispositions, have tended to confound the denial of this thesis with denial of another one. This latter, their ontological thesis, denies the existence of certain alleged non-human entities such as societies or world-spirits. They correctly point out that belief in the reality of such beings is entirely without foundation, and then seem to think that this establishes the methodological position they wish to uphold.

We are now informed by Mr Watkins that there was no need for me to announce 'with an air of making a discovery' that there are metaphysical as well as methodological issues at stake. Watkins knew it all the time, as anyone who consults the proper paper³ can see for himself. But having made this 'small historical comment', as he calls it, Watkins has said nothing that bears upon my paper. I argued that attempts to defend methodological individualism reflect a failure to distinguish between the methodological and ontological theses of its proponents. If Watkins is sensitive to the distinction, one can only wonder why his defence of his views does not reflect it.

He then goes on to summarise his views in two propositions, which I quote:

(1) Human beings (together with their material resources and environment) are the only causal factors in history.

(2) Explain all social events in terms of human factors.

He acknowledges that I subscribe to (1), but then says that I replace (2) with:

(2') Explain some social events in terms of non-human factors.

But since (1) and (2') are not compatible—(2') prescribing what (1) precludes—I am presumably in a somewhat untenable position.

Happily, I am easily able to extricate myself, for I need only assert what is, in fact, the truth: that I have never affirmed (2') and do not affirm it now. But it is

¹ This *Journal*, 1959, 9, 319-320

² This *Journal*, 1958, 9, 1-11

³ 'Ideal Types and Historical Explanation' in *Readings in the Philosophy of Science*, ed. Feigl and Brodbeck, pp. 731, 732.

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interesting to note how incapable Watkins is of understanding the denial of his methodological thesis—which is what (2) is—except as the denial of his ontological thesis—which is, of course, what (1) is. Yet that is precisely the point of my paper to which he takes exception.

Since I deny that (2') is a view I hold, I suppose it is incumbent upon me to try again to express what I take the alternative to Watkins's methodological thesis to be. Social science, as indeed any science, is made up of propositions which embody concepts and not existential entities, human or other. I take it that the issue between individualism and non-individualism in social science concerns the proper way to analyse these concepts. The individualist claims that all sociological notions may be analysed without remainder into individualistic concepts or biopsychological concepts of a certain kind. This is what the non-individualist denies. In his 'Societal Facts', Maurice Mandelbaum defends the non-individualist thesis and by way of example shows that the social content of the notion 'bank teller' cannot be analysed as per the prescription of methodological individualism.¹ It is, I think, incumbent upon methodological individualists to offer an alternative analysis of this simple notion if they want to insist upon their view. And a flippant 'only individual human beings can be bank tellers' won't do. That the values of the sentential function '... is a bank teller' can only be human beings, biopsychological entities of a certain kind, does not entail that the social content of 'bank teller' can be analysed without remainder into individualistic or biopsychological terms. Which is only another way of saying that Watkins's ontological thesis does not entail his methodological one.

As much difficulty as the task of attempting an individualistic reduction of such simple notions as 'bank teller' would afford the methodological individualist, so much more would be the difficulty in trying to treat more theoretical notions in such a manner. Elsewhere,² I have argued that the theory of kinship systems in G. P. Murdock's *Social Structure* cannot be treated in accordance with the individualists' prescription. While Watkins insists that I am mistaken, he has made no serious effort to show this. He could show it only by offering an individualistic analysis of the theory. In what purports to be a rebuttal of my earlier paper,³ he may think that he has said all that is needed, but this is not the case. To begin with, he changes the subject under discussion, for the example I give is not concerned with 'an anthropologist who has set himself the task of describing the structure and nomenclature of a certain kinship system' (p. 394), but rather with a proposed anthropological theory which it is hoped will explain the development and change of such systems. And second, what is actually analysed by Watkins (p. 394 f.) is not Murdock's theory but a rather inadequate one of his own devising.

If, as Watkins says, methodological individualism is an 'excellent principle', it is only to be regretted that he has never shown that it is.

LEON J. GOLDSTEIN

¹ *British Journal of Sociology*, 1955, 6, 305-317

² 'The Inadequacy of the Principle of Methodological Individualism', *Journal of Philosophy*, December 6, 1956, 53, 801-813

³ 'The Alleged Inadequacy of Methodological Individualism', *Journal of Philosophy*, April 24, 1958, 55, 390-395. This purported rebuttal is actually full of misinterpretation and changing the subject.

THIRD REPLY TO MR GOLDSTEIN

THERE can be no doubt about it: Goldstein has caught me out this time. I made a real howler. I naively supposed that since he was attacking my position he must disagree with it. And since I already knew that he accepted my metaphysical proposition (1), I erroneously concluded that it could only be my methodological prescription (2) which he rejects. But I was wrong, as Goldstein rightly insists.

There were by no means exculpating, but perhaps extenuating, circumstances which help to explain, though not to justify, my error. Goldstein's views do at least *sound* as if they conflict with mine. He tirelessly attacks methodological individualism: 'social science must', he says, 'be non-individualistic' (this *Journal*, 1958, 9, 3). He speaks of 'sociological emergence' (p. 6) and of 'laws of social change' (p. 10). But a philosopher ought not to be misled by mere appearances. Instead of lazily taking at its face value Goldstein's insistence that social science must employ non-individualistic, sociological concepts, I ought, of course, to have asked myself: To what can Goldstein suppose that such concepts refer? If only I had paused to ask myself this, I would not have made the mistake of assuming that he dissents from my own individualistic methodology. For he cannot suppose that they refer to non-individualistic, social *entities*, for that would be metaphysical holism which he rejects; nor to social or historical *forces*, for that would be historicism which he also rejects. The only thing that he *can* suppose these non-individualistic concepts to refer to, I now belatedly realise, is individuals. I see now that this is indeed Goldstein's position, though it has a subtle complication: the individuals to which these sociological concepts refer are not ordinary individuals, like him and me, but *anonymous* individuals who, because of their anonymity, are not exactly individuals after all, but rather constituents of *societies*.

There was once a time when Goldstein believed that methodological individualism requires the social scientist to impute social happenings to individuals he can actually identify by name. I explained that this was not so. No methodological rule is transgressed by a financial columnist who writes that share prices fell yesterday because investors (no names given) were feeling uneasy about the situation in the Middle East.

Goldstein recognises this now, though he still feels that methodological individualists *ought* to confine themselves to individuals identified by name. I think he feels that we are somehow *cheating* when we introduce anonymous individuals. 'The notion that we can talk about the dispositions of anonymous people seems to me', he says, 'somewhat strange' (p. 11). Well, fact may be stranger than fiction; and it still seems to me to be a fact that we can talk about the thriftiness of Scotsmen without mentioning their names. 'There is no science of the anonymous', Goldstein declares. I was rather under the impression that there was not much science of anything else. I thought it was rare for proper names to figure in scientific theories. When I did chemistry at school, the atoms whose dispositions we learned about were classified into types but they were not individually identified by name. But perhaps things are different now.

Anyway, Goldstein's idea is that when we stop talking about Charles I and Cromwell and start talking about the characteristics of Cavaliers and Roundheads, we stop talking about individuals and start talking about the non-individual char-

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acteristics of parts of a society. 'I venture to suggest that wherever Watkins talks about anonymous dispositions or the dispositions of anonymous individuals, he is simply attempting to talk about the non-individual characteristics of societies, or parts of societies . . .' (p. 11). Very well. Suppose that I do stop talking in my old and misleadingly circumlocutory way about Scotsmen and investors and start speaking in a direct and straightforward way about the non-individual thriftiness or uneasiness of this or that part of society. We must remember, though, that Goldstein insists that a society, metaphysically or ontologically speaking, is a collection of individuals. So that if I did reform my way of talking along Goldstein's lines, he could at once accuse me of talking in a misleadingly short-hand way about the characteristics of unidentified individuals, or perhaps about the social results of the interaction of unidentified individuals. I fear I shall never please him.

Now that the scales have fallen from my eyes, I realise that none of those anti-individualistic sounding terms which Goldstein flourished ought to have misled me. 'Sociological emergence' is his word for what I would describe as the large-scale and mainly unintended social results of the interacting small-scale activities of individuals. And the kind of social 'law' in which he believes is quite innocent since it 'proclaims no necessary successions' (p. 10). It is not an iron law but an india-rubber law. Since individuals can violate it, it does not violate methodological individualism.

And yet—Goldstein has attacked me three times (and he refers to a fourth onslaught). Can it really be that there is *no* fire behind all this smoke? In my previous two replies I have complained about four misrepresentations of my views (little realising that this would boomerang and that *he* would complain about my misrepresenting him as disagreeing with my methodology). If I now, chastened as I am, mention a fifth misrepresentation, it is because I believe that this explains why he attacks my methodology although he does not disagree with it.

In his current rejoinder, he writes: 'The individualist claims that all sociological notions may be analysed without remainder into individualistic concepts or biopsychological concepts of a certain kind.' So far as I know, *no* methodological individualist has ever claimed such a thing. Just as the practitioners of an individualistic method, from Adam Smith to Keynes, have been concerned to understand and *explain* social happenings (often with an eye to their remedy), so have advocates of an individualistic method, such as Hayek, Popper, and myself, been concerned with right and wrong *ways of explaining* social happenings. But I now think that I, at any rate, was guilty of a considerable blunder. I should have realised, when I insisted that all large-scale social *happenings* are, in principle, individualistically *explainable*, that there are many social methodologists who concern themselves with the uninteresting question of analysing sociological concepts rather than with the interesting question of ways of explaining what those concepts describe, and that these methodologists would naturally tend to mis-read me as insisting that all sociological *concepts* are individualistically *analysable*. My blunder consisted in failing to take adequate precautions against this fashionable sort of mistranslation into the formal mode. If I had, Goldstein would hardly have thrown down the challenge to methodological individualists to offer an individualistic analysis (whatever that might be) of 'bank teller'. We just are not, and are not obliged to be, interested in such verbal exercises (though we do have something to say about what a good explanation of the working of a banking system would look like).

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The truth appears to be that Goldstein does not dissent from either (1) the metaphysics or (2) the methodology of individualism, but he does object to a linguistic prescription (3): 'Analyse all sociological concepts individualistically.' Since none of us has ever prescribed (3), I am a little doubtful whether his objection to it altogether justifies the heavy and prolonged offensive he has mounted against our position.

With my eye on Goldstein's last sentence, I will end by remarking that if, as he says, methodological individualism is no good, it is only to be regretted that he has never criticised it.

J. W. N. WATKINS

REVIEWS

The Language of Modern Physics: An Introduction to the Philosophy of Science.

By Ernest H. Hutten.

George Allen and Unwin, London, The Macmillan Company, New York, 1956. Pp. 278. 21s.

THE aim of this book is to provide a reasonably complete account of the major theoretical concepts of both classical and quantum physics. However, Dr Hutten believes that the intellectual achievements of science cannot be properly understood without familiarity with some of the analytical tools and the conclusions developed in the philosophy of science. The first part of the book is therefore an exposition of basic distinctions made in modern logic and semantics, while its concluding portion discusses fundamental issues of scientific method. The three central chapters which constitute more than two-thirds of the book employ those distinctions in surveying the main theoretical ideas of Newtonian mechanics, electrodynamics, relativity theory, thermodynamics, and quantum mechanics. Dr Hutten seeks to promulgate no new philosophical doctrines, although he does attempt to show that various philosophical interpretations (traditional as well as contemporary) of scientific theories and principles are either irrelevant or mistaken. His inclusive objective is to make evident that 'The scientific attitude is to solve problems, and if unsuccessful, to start anew, always guided by a hypothesis and corrected by experiment'; and he therefore stresses, in opposition to crude 'empiricistic' accounts of science, the hypothetico-deductive and self-corrective character of scientific method. His book is a persuasive argument for these eminently sensible conceptions. It contains not only much that the layman in physics and its philosophy will find instructive, but also clarifying discussions of special points that are addressed to the professional student. For these reasons I feel all the more apologetic to the readers of this *Journal*, and not only to Dr Hutten, for the long delay in preparing this notice and calling their attention to the book.

Since according to Dr Hutten a theory is a complex language which is used for the 'description of experiments' and for predicting new ones, he first explains some of the notions of current formal logic and semantics on the ground that they are indispensable tools for analysing the structure and the conditions of applicability of theories. His exposition of those notions is admittedly 'neither very adequate nor entirely accurate', and it would therefore be gratuitous to note unclarities and dubious formulations in his account. In my judgment, however, he exaggerates the importance of

modern formal logic and semantics for the philosophy of science. It is certainly debatable whether the extensive use of those notions as tools of analysis by recent students of the philosophy of science has yielded fruits comparable in the illumination they provide with the unformalised discussions contained in the writings of Mach, Duhem, and Poincaré, or in the more recent publications of Campbell, Bridgman, and Frank. Moreover, although Dr Hutten employs in the sequel the terminology he borrows from current formal semantics, I do not believe that anything in clarity is gained as a consequence.

In Dr Hutten's view, a physical theory can be analysed into a formal (uninterpreted) calculus that constitutes the logical skeleton of the theory, and a set of semantic rules (sometimes called 'co-ordinating definitions' or 'rules of correspondence' by other writers) that establish some connection between the non-logical terms of the calculus and experimentally identifiable traits of a subject-matter. Dr Hutten rightly stresses the indispensability of such rules (subject to an important qualification to be noted presently), since without them no theory could in principle be put to an experimental test. Nevertheless, he seems to me to oversimplify matters even in the case of an ideally formulated theory, when he maintains (or appears to maintain) that semantic rules of designation and of truth must be provided for all the primitive terms and all the axioms of a theory (p. 43). As his own discussion of the kinetic theory of gases as well as of quantum mechanics amply shows, what is essential is that such rules be available for *some* theoretical terms (not necessarily the primitive ones) and for *some* theoretical sentences (not necessarily the axioms). In any event, in explaining the rôle of co-ordinating definitions he is inevitably compelled to discuss the troublesome question as to what is to be understood by 'observation' and what is to count as observable in physics. Although he makes many excellent comments on this problem—among others, in dismissing the realist-nominalist issue as irrelevant, and in characterising the phenomenalist thesis as the product of a misguided search for certainty—in the end I have not found his discussion very helpful. He declares that 'A thing or event is observable, directly or indirectly, if we can devise tests for the observation-sentence which refers to the thing or event' (p. 53)—a conclusion which places a heavy burden on the inadequately analysed notion of 'testable'. Unless I misunderstand Dr Hutten's intent in this connection, he is required to say that for the nineteenth-century physicist the aether is observable, just as for contemporary physicists electrons said to be observable. It is no doubt possible to assent to such statements when they are suitably construed. But such assent does not come near the root of the questions under analysis. I also find myself in substantial agreement with Dr Hutten's strictures on various 'theories' or 'criteria' of meaning. Nevertheless, I have been unable to extract much nourishment from his positive suggestion

that the construction of an adequate criterion of meaning requires the 'formalization' of language and the use of semantic methods (p. 68). Even if one waives the question whether our everyday language (or for that matter, the language of physics) can be fully formalised, it is plain that unless a proposed formalisation is quite arbitrary, we must understand at least in part the meanings conveyed by that language. Accordingly, I fail to grasp the rationale for his contention that 'If someone asserts that he has seen a ghost, it is a *literally* meaningless sentence that he utters', since 'we do not know, strictly, how to use the word "ghost"' (p. 71, *italics* in the text). The word 'ghost' may indeed have no precise meaning, and we certainly possess no explicitly formulated semantical rules for its use; but it is surely not meaningless. Nor does formalisation appear to provide an automatic panacea for linguistic obscurities, unless the effective resolution of all difficulties concerning the meaning of expressions is taken to be part of what is to be understood by 'formalization'. For example, although Dr Hutten claims that physicists have succeeded in formalising some of their theories, it is notorious that even in the most successfully formalised theories the precise meanings of some crucial theoretical concepts are still debated by competent students of the subject.

Dr Hutten is fully aware, however, that for most physical theories there are no explicitly formulated semantical rules. Indeed, those parts of his book that seem to me by far the most illuminating deal just with the problem raised by this circumstance. In his account of the rôle of models in physical theory he notes the psychological values familiarly attributed to them. But he also makes the important and generally neglected point that models have the crucial logical function of indicating how various expressions in a theory for which no precise semantical meanings have been assigned (even though such expressions are exact mathematical formulas) may be applied to concrete experimental situations. As he puts the matter, 'We may not be able to give the [semantical] rules in an explicit form, but we can show them by giving a model' (p. 169). He illustrates this point by a large assortment of examples drawn from classical and quantum physics; and he thereby shows convincingly how theories that would otherwise have only tenuous connections at best with experimental subject-matter are made testable with the help of models functioning as co-ordinating definitions. Moreover, once he has established the logical rôle of models, he can unravel systematically a number of puzzling features of recent physical theories. For example, he exhibits the relations of dependence between classical and quantum mechanics, in terms of the models employed in these branches of physics. He thereby shows clearly how the use of two apparently incompatible models in quantum theory (the particle and the wave models) turns out to be a fruitful instead of an absurdly self-contradictory procedure, and why that procedure leads to Bohr's principle of complementarity. In short, Dr

Hutten's account of the rôle of models seems to me a substantial contribution to our understanding of the logic of theoretical science. His account would have been even more valuable if he had said explicitly and in some detail what kinds of models can function as semantical rules. As it is, he sometimes gives the impression that any model can serve in that capacity, a claim that is surely untenable whether or not Dr Hutten would be prepared to make it. To cite an extreme example, a model can be supplied for the well known postulates for linear order, in terms of an assumed band of angels who form a hierarchy according to their degrees of relative holiness. This model, at any rate, does not suffice to convert the postulates into a testable theory. It is therefore pertinent to ask what conditions must be satisfied if a model for a theory is to perform the logical part which Dr Hutten has helped to identify.

Dr Hutten follows fairly familiar lines of analysis in his discussion of the statistical interpretation of quantum theory, and is obviously especially indebted to Reichenbach's publications on the subject. In the light of his earlier statements on what is to count as observable, it is somewhat surprising that he appears to accept without critical scrutiny Reichenbach's crucial but debatable distinction between 'phenomena' and 'interphenomena'. On the other hand, Dr Hutten makes the excellent point that although classical mechanics can be derived as a limiting case from quantum mechanics (for which the currently accepted model is a statistical one), it is a mistake to claim, as many commentators have claimed, that all the laws of classical mechanics are therefore 'at bottom' really statistical.

I have already mentioned Dr Hutten's emphasis on the hypothetico-deductive nature of scientific procedure. In his general account of scientific method he not only supplies a good statement of the character of the hypothetico-deductive procedure, but also discusses several other crucial concepts, among them that of probability. He recognises two apparently irreducible senses of 'probability': the relative frequency notion of current statistical theory; and 'inductive' probability as the degree of support that given evidence provides for a stated hypothesis. Dr Hutten takes inductive probability statements to be analytic, so that on this and a number of other issues which are of special relevance to the analysis of scientific method he seems to accept the approach represented in Carnap's writings on inductive logic. Accordingly, the unresolved difficulties in Carnap's approach are also present in Dr Hutten's version of it. Moreover, some of his statements about inductive probability present difficulties of their own. For example, he declares that if a hypothesis is a statistical one and therefore concerns a frequency (as in the case of the hypothesis that the relative frequency of obtaining heads with a repeatedly tossed coin is one-half), 'the text-books on Statistics give many methods for evaluating the probability of hypotheses of this type' (p. 261). But unless the quotation contains a serious misprint,

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Dr Hutten's assertion is mistaken. Statistical theory does indeed offer ways of estimating an unknown relative frequency from known data; however, statistical theory that is based on a relative frequency interpretation of probability does not, and in the nature of the case cannot, provide measures for the inductive probability of hypotheses formulating such estimates.

I have not found it possible to give an exhaustive account of the interesting things Dr Hutten discusses in his book; and I have also found it necessary to indicate some of my reservations concerning several of Dr Hutten's views which I have selected for mention. But I hope nonetheless to have given enough evidence for the wealth of materials, the generally high quality of the presentation, and the frequently illuminating character of the analysis to be found in this book.

ERNEST NAGEL

Hypothèse du continu. Second edition. By Wracław Sierpiński.

Chelsea Publishing Company, New York, 1956. Pp. xvii + 274.
\$4.95.

It is exactly fifty years ago that Zermelo and Russell, independently, published axiom systems for two versions of Cantor's theory of sets, each of which provides a basis for proving nearly all known theorems of pure mathematics, and both of which appear to be formally consistent. Much later, it was proved by Gödel that no such axiom system can be complete (that is, can have the property of providing, for each statement expressible in the terminology of set theory, either a proof or a refutation). Therefore, mathematicians will always try to extend such axiom systems by adding suitable statements as axioms. And, before they commit themselves in this respect, they will try to find out what the consequences are if, provisionally, they add such a statement as a hypothesis. If a contradiction results, then the negation of the statement under consideration follows from the original axiom system; if very implausible consequences arise, it will not be advisable to promote the hypothesis to the rank of an axiom; but if the consequences are interesting and reasonably plausible then its introduction as an axiom can be permitted.

Sierpiński's book, first published in 1934 as volume IV of the *Monografie Matematyczne*, is mainly devoted to the study of Cantor's so-called *continuum hypothesis* which expresses the supposition that the cardinal number of the set of all real numbers is the smallest cardinal number larger than the cardinal number of the set of all natural numbers. Certain statements are shown to be equivalent to the continuum hypothesis; many others follow from the continuum hypothesis, but are not known to entail it. Other prospective axioms, discussed by Sierpiński in a similar manner,

are: the hypothesis concerning the *inaccessible alephs*, and the *generalised continuum hypothesis*.

Since 1934, considerable progress has been made in the field covered by Sierpiński's book. On the one hand, more statements were proved to be equivalent to the continuum hypothesis or to follow from it; on the other hand, Gödel (1938) has shown that both the generalised continuum hypothesis and the axiom of choice are consistent with the main body of current axiom systems for set theory.

The present volume consists of a photographic reprint of the first edition and of sixteen articles, published by the Author between 1935 and 1954 and containing new results. As the book is an outstanding specimen of modern abstract thought, the new edition has the great merit of making it more accessible and bringing it up to date.

E. W. BETH

The Scientific Study of Social Behaviour. By Michael Argyle.

Methuen, 1957. Pp. xiii + 239. 21s.

THIS book is a manual of the methods and current results of social psychology and contains a large amount of detailed information which will be useful to the social research-worker. Part I presents what might be called the minute methodology of the subject. It discusses the merits of the scalogram board, the non-directive interview (Dr Argyle regards a psycho-analytic session as a somewhat inefficient 'interview'), the questionnaire, open-ended questions, the interaction recorder, sociometric techniques, attitude scales, the concealed observer (a faintly sinister figure) and numerous other pieces of laboratory equipment. Questions of a more philosophical interest are only touched on—not always in a manner sympathetic to this reviewer. The author rightly stands for no nonsense about social wholes; but he half commits himself to that trio of scientific shibboleths, social determinism, behaviourism, and the idea that psychological conclusions can be deduced from physiological premisses.

The book is written in what is surely a deliberately dead-pan style, presumably in devotion to some scientific ideal. This is a pity because it makes the terrain seem flatter than it is. There is a massive bibliography and the book's pages are studded by references to it—for instance, 'The principal exponents of this approach are Snygg and Combs (1949, 1950); Krech and Crutchfield (1948) claim to be following Lewin, but proceed throughout in the Snygg manner.' Some references seem superfluous. Was there any need to refer to Underwood (1949) after a simple account of what an experiment is? His citations in support of his larger methodological contentions do not always inspire confidence. Thus he quotes Mill's accounts of the Methods of Difference and Concomitant Variation and

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says that these 'two are the most important' for social psychology. But he does not cite the later passage where Mill gives cogent reasons for concluding that in social enquiries these two methods are 'completely out of the question'.

Parts II and III present a mass of small achievements in social psychology, mostly arrived at since 1940. I have space to mention briefly only one of the interesting experiments described. To assess the relative importance of personality and institutional rôle as determinants of institutional behaviour compare the behaviour of newly promoted officers (*a*) with their previous behaviour and (*b*) with that of their predecessors in their new posts. A few of the findings reported here are frankly dull ('Murphy found that insecurity increased sympathetic behaviour in some children and reduced it in others'). But it would be wrong haughtily to dismiss all this as laborious pedantry pretending to be science. I am persuaded that more insight into the make-up of human life is to be gained by reading biographies and histories and Dickens and Dostoevsky and Proust than by reading the books and articles mentioned in Argyle's bibliography. Nevertheless, many of the findings he presents provide very useful correctives to the confident judgments of lazy commonsense on such important practical matters as incentives, methods of selection, and absenteeism.

J. W. N. WATKINS

Foundations of Statistical Learning Theory, 1. The Linear Model for Simple Learning. By W. K. Estes and Patrick Suppes.

Technical Report No. 16, Applied Mathematics and Statistics Laboratory, Stanford University, 1957. Pp. vi + 99.

LEARNING theory today is one of the most important and respected of the American schools of psychology, and is essentially based on learning by reinforcement. Both Bush and Mosteller and Estes and Burke have recently made systematic attempts to present probabilistic (or statistical) analyses of the data obtained in learning experiments. We are given mathematical models which, so it is claimed, are at once predictive and operational in character.

These accounts though worded in probabilistic (or statistical) terms are based on the learning studies of Hull and Guthrie and employ the classificatory categories of these avowedly causal theories. This is reflected in the selection of the variables to be sampled and correlated, i.e. in the choice of the system's units—stimuli, responses, and environmental events. Though we are given a probabilistic analysis of behaviour, it is behaviour as seen from the rather restricted standpoint of the learning theorist.

A fundamental criticism of this kind of approach is that it assumes that causal accounts of behaviour in terms of motivation have exhausted their

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usefulness. However, one of the tasks of psychological science is to discover the causal mechanisms underlying behaviour. We look to our psychological theories to explain as well as predict.

In this mimeographed report of some one hundred pages, Estes and Suppes put forward a linear model for simple learning in terms of axiomatic set theory. An important feature of their conceptual model is that it is of sufficient generality to include both the probabilistic system of Bush and Mosteller and the statistical learning theory of Estes and Burke as special cases.

Simple learning on this theory is taken as a change in the behavioural probabilities which occur as a function of a certain number of trials. In these trials the organism responds to the stimulus with one or another of a set of alternative actions. Environmental events which increase the probability of a given response, such as rewards, are termed reinforcing events, whilst those which leave the response probabilities unchanged are instances of non-reinforcement. The amount of change in probability is determined by the environmental events and the work or effort the organism expends in making the change.

The report contains little by way of explanatory discussion and the treatment is largely symbolic in character in the best *Principia Mathematica* tradition. The reader who has not some previous acquaintance with learning theory, symbolic logic, and stochastic models, will find it hard going unless he makes himself acquainted with the relevant literature.

W. MAYS

Pain and Pleasure: A Study of Bodily Feelings. By Thomas S. Szasz.
Basic Books, New York, 1957. Pp. xviii + 301, \$5.50.

THE core of this timely and stimulating book consists of revised versions of a number of papers which were previously published in various psychiatric and psycho-analytic journals. In these papers, Dr Szasz, an American Professor of Psychiatry and a psycho-analyst, had discussed certain aspects of the experience of pain and bodily feelings. To these he has now added a chapter on phantom limb phenomena, and two chapters on pleasure. In an introductory section he considers the mind-body problem, and in a final section, he adds some remarks on the sociological implications of the concepts of pain and pleasure. Dr Szasz has many original and illuminating ideas to offer, but it is a pity that he did not put his earlier papers aside, good as they are, and set out afresh on the whole problem. One feels that this would have led to a tighter and more systematic approach to his theme, without the repetition of basic assumptions and the interspersed commentaries on the literature which weaken the structure of his argument.

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The thesis of the book is that pain, pleasure, and bodily feelings are affects which may or may not refer to the body. Szasz shows how pain and pleasure may be defined in a variety of ways—as sensation, perception, affect, signal or object. Depending on which concept is used, so one asks different questions about the origin and function of the phenomenon, and uses different methods in seeking the answers. Aligning himself with Russell's view of psychology, Szasz considers that bodily feelings, whether painful or pleasurable, may be regarded as both private and public data. He argues that the dualism of body and mind can be avoided if it is recognised that the ego is oriented always towards two environments—the body and other persons. From the point of view of the experiencing ego, all pain refers to the body. This highlights the fact that the terms 'organic' and 'psychogenic' do not refer, strictly speaking, to the pain experience at all, but describe the judgment of the observer about the source or location of the pain. Likewise, the terms 'objective' and 'subjective' refer to judgments by an observer as to the cause of an experience, and not to the experience itself.

By treating pain as an affect, Szasz is able to relate it closely to the psycho-analytic theory of internal signals of anxiety, and to extend certain ideas from ego-psychology about the relation between ego and self, and between body and self. Whereas the ego responds with anxiety to the threat of object loss, it responds with pain to the threat of danger to the body: there being two possible meanings for the pain signal—danger of loss of part of the body, and danger of excessive stimulation.

In a valuable chapter on the symbolic meanings of pain, Szasz extends this primary model of pain as communication between body and ego—to consider pain as a social phenomenon. Through learning, pain as an originally biological signal comes to mean 'asking for help', and may extend symbolically to represent various complex demands made upon other persons, whether they be in the past or present. (Indeed, recent work by Melzack and Scott at McGill makes it very doubtful whether Szasz's primary biological meaning of pain can ever be shown to exist, since dogs reared in 'pain-free' environments were subsequently deficient in the capacity to perceive and respond to pain.)

Szasz's view of the interplay between ego and body, with all the social components, both conscious and unconscious, of the ego's image of the body, leads him to an interesting comparison between the experience of phantom body parts and other states of personal loss. As in mourning for the dead we work through the trauma of object loss by means of a process of mental resurrection, so the victim of body loss may be said to revive the lost part, but with hallucinatory vividness. By a similar argument, the 'noisy claim' of phantom limb pain may be compared with the demands of persecutory delusions in paranoia. In the same way that mourning experiences and persecutory delusions represent, in part, attempts to reclaim

lost persons, so the persistent quality of phantom pain entails the ultimate denial that a body part has been lost. As 'compromise formations', all these states transmit a double message : of felt loss and of desired presence. These analogies should provide valuable leads in the study of individual differences in phantom limb and pain experiences, for although about ninety-five per cent of people who have lost an arm or leg have the phantom experience, only about thirty per cent of those with phantoms suffer pain in them. It should be possible to examine the relation between certain personality defence patterns, and the nature of phantom experiences.

Szasz's discussion of the concepts of pain and pleasure should do much to stimulate work which might co-ordinate physiological and psychological approaches to the study of reactions to traumata. In a general way, he is to be congratulated on the extent to which he has clarified the meaning of fundamental concepts, confusion about which has muddled and held back much theoretical and empirical work in medicine and psychology.

CECILY DE MONCHAUX

The Copernican Revolution: Planetary Astronomy in the Development of Western Thought. By Thomas S. Kuhn.

Harvard University Press, Cambridge, 1957. Pp. xx + 297. 45s.

THIS book is the outgrowth of a series of lectures delivered each year since 1949 at Harvard; its author now teaches the History of Science in the University of California. He was attracted to the theme of the Copernican Revolution by its very complexity, curious to discover how the concepts of many different fields—astronomy, cosmology, physics, philosophy, and religion—could be woven into a single fabric of thought.

The Revolution centred in astronomy; and in his first three chapters, with their technical appendix, Professor Kuhn explains the underlying celestial phenomena and the classical world-view—the 'two-sphere universe'. He sees in many of Aristotle's substantive ideas (as distinguished from his logical method and abstract vocabulary) important primitive residues which can be paralleled in the world-views of children, savages, and mentally regressive patients.

A chapter on the medieval recasting of the ancient cosmological tradition bridges the fourteen centuries from Ptolemy to Copernicus and prepares the reader for the analysis of the basic ideas of the *De Revolutionibus* which forms the core of the work. For Professor Kuhn, 'the significance of the *De Revolutionibus* lies . . . less in what it says itself than in what it caused others to say It is a revolution-making rather than a revolutionary text (It) stands almost entirely within an ancient astronomical and cosmological tradition; yet within its generally classical framework are to be found a few

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novelties which shifted the direction of scientific thought in ways unforeseen by its author and which gave rise to a rapid and complete break with the ancient tradition' (p. 134). There follows an account of the triumph and transformation of the Copernican theory through the contrasting achievements of Tycho Brahe, of Kepler, and of Galileo, in the face of opposition, partly scientific, partly derived from 'a subconscious reluctance to assent in the destruction of a cosmology that for centuries had been the basis of everyday practical and spiritual life' (p. 226).

The concluding chapter traces the progressive removal of the immense conceptual disparity between Copernican astronomy and the traditional assumptions implicit in neighbouring branches of science and philosophy. Professor Kuhn sees a close historical parallel between this process and the twentieth-century revolution in physics: the paradoxical concepts of Planck and Einstein, introduced to solve pressing problems in a single field of science, soon transcended their function of describing the known and became basic tools for exploring the unknown. But at this stage they could not be confined to a single science: 'every fundamental innovation in a scientific specialty inevitably transforms neighbouring sciences and, more slowly, the worlds of the philosopher and the educated layman' (p. 230).

By his thorough exposition of the technical issues, his extended and illuminating quotations and his evaluation of the philosophical implications of the great controversy, Professor Kuhn has succeeded in bringing vividly to life what was at once an episode in the internal development of astronomy, a turning-point in the evolution of scientific thought, and a crisis in Western man's conception of his relation to the Universe and to God. The book is documented, and it concludes with a reasoned reading list (incidentally, the biographer of Copernicus cited on page 287 was *Leopold* not *Ludwig* Prowe).

A. ARMITAGE

In Search of Reality. By Viscount Samuel.

Blackwell, 1957. Pp. viii + 229. 28s. 6d.

LORD Samuel explains in the preface to his latest book that it is addressed to the ordinary reader and warns him that realism is not to be identified with materialism. The ordinary reader, if he is the same as the man in the street, tends to be a realist; a somewhat naïve one who, when driven into an argumentative corner, falls back upon the scholastic apparatus of substance and accident. If he reads Boswell, he immediately understands Johnson's refutation of Berkeley, particularly as it was Johnson who rebounded from the stone. In recording this anecdote Boswell says that the 'nice and difficult task' of answering Berkeley 'was to have been undertaken by one of the most luminous minds of the present age, had not politicks "turned him from philosophy aside"' and goes on to quote Goldsmith's *Retaliation*,

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'Who, born for the universe narrowed his mind, And to party gave up what was meant for mankind'. Happily the parallel is not exact: Lord Samuel has outlived Burke by twenty years to become an elder statesman; he has remained a faithful exponent of a tolerant political creed; and, in the later lustre of a long life he has returned to an early love whose acquaintance he must have made in Jowett's Balliol.

Lord Samuel searches for reality as a philosopher rather than as a scientist: the last seven chapters of his book deal with life as it is lived and discuss, for example, mind, imagination, human institutions, and religion; the first five examine certain problems raised by the physical sciences and are more likely to interest readers of this Journal, though they will be put to the trouble of reading them in conjunction with the lengthy appendix to which have been relegated matters which the author thinks may be too technical for the public he has in mind. The difficulties are stated in Chapter III: they are the contradiction implied by wave-particle mechanics, the whence and whither of appearing and disappearing particles, the transmission of wave patterns, the nature and conveyance of gravitation, the denotation of the word *impetus*. Chapter IV submits a proposition 'that there must exist, as a fundamental element in the real universe—underlying all its phenomena but itself imperceptible and as yet unrecognized—a Continuum, universal and perpetual. This is the medium that conveys all radiations, including gravity. It may also be the seat of motive force.' Before introducing his speculation in Chapter V Lord Samuel remarks that 'the layman, bringing to the survey of old problems an innocent mind, not hampered by too much erudition, may sometimes happen upon a clue—perhaps a very simple one—which specialists may find helpful'; and the speculation is that 'if there is a universal Continuum—an ether, and if the ether consists of energy, we may conceive that energy may exist, not only in a variety of Forms—as we all know that it does—but also in more than one State and in several Patterns.' The thesis of the smaller book published in 1951, *Essay in Physics*, is discussed and revised: it is 'that the Continuum is an ether consisting of energy' which may be either quiescent or active and that 'the phenomena we perceive may come from activations of quiescent ether or relapses'. Lord Samuel does not claim uniqueness for this ether and in a world which is now being introduced to the notion of anti-matter his speculations will not seem strange. But the self(?)-activation of this tideless and inert ocean needs a new Kelvin to invent for us a model.

There have been many books whose titles comprised the word *Reality* and there will be many more. There is a sense in which each of us conducts his own search and it is unlikely that any one's reality is any other's, though we should all agree that the *realitas* we seek must also possess *veritas*.

I have one complaint: the book is addressed to a wide public but 28s. 6d. is an unrealistic price.

E. ROWAN DAVIES

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A B C of Relativity. By Bertrand Russell

George Allen and Unwin, London, 1958. Pp. 139. 15s. net.

THE revised edition of this book, originally published in 1925, contains ten new pages (on the Expanding Universe) and various small additions by Mr F. Pirani. It is an elementary, non-mathematical introduction to the special and general theory of relativity—full of homely illustrations and mild jokes. The following sentence is characteristic of the whole book: 'But if the escalator moved with the velocity of light (which it does not even in New York), you would reach the top at exactly the same moment whether you walked up or stood still.'

The passage of more than twenty years has not, I believe, affected the usefulness of this introduction, especially its capacity to whet a young reader's appetite for reading more advanced texts on the subject and for acquiring the necessary mathematical equipment.

S. KÖRNER

Science and Metaphysics. By John Russell, S.J.

Pp. 35. 2s. 6d.

Life and Its Origin. By Philip G. Fothergill.

Pp. 70. 3s. 6d.

Whitehead's Philosophy of Physics. By Laurence Bright, O.P.

Pp. 40. 2s. 6d.

Newman Philosophy of Science Series, 1, 2, and 3. Published under the auspices of the Newman Association of Great Britain (Philosophy of Science Group) by Sheed and Ward, London, 1958.

IT is not easy after reading these first three books to decide for whom this new series is intended. The first, Father Russell's *Science and Metaphysics*, seems to be written primarily for the sixth form of Catholic grammar schools; for, understandably, this thirty-one page discussion is at a very elementary level. The author simplifies his task by confining himself to scholastic metaphysics whose function he characterises as that of elucidating the meaning of 'the most fundamental and universal terms of rational discourse'. His conclusions are that metaphysics and science each have their own distinctive method, are each autonomous in their own field, are both necessary for complete knowledge, and need never conflict with each other. Such an approach will disappoint anyone who is hoping for some discussion of the interaction of scientific and metaphysical ideas. In fact the book contains little more than two separate short essays on science and scholastic metaphysics connected only by the contrast which is drawn between the univocal concepts of the former and the equivocal concepts of the latter.

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Dr Fothergill's book is quite different. It is valuable more for its interesting, though somewhat crowded, survey of some of the different theories about life than for its contribution to the philosophy of science. The general conclusions which he draws are unexciting: that scientists have not yet explained the origin of life and that the concept of life cannot be defined solely in terms of physical and chemical concepts.

Father Bright's discussion of Whitehead is in many ways the most satisfying of the three books. For it is a careful essay which expounds with considerable clarity a part of Whitehead's philosophy. But however lucid the exposition, no presentation of Whitehead can hope to be easy nor is it likely to be comprehensible to someone who has no previous knowledge of philosophy. It is thus difficult to conceive those who will appreciate the first book of this series being able to benefit much from this one.

The three books are printed on poor quality paper and stuck in primrose yellow paper covers so that their appearance is that of religious pamphlets. This was presumably the only way in which their price could be kept so excellently low; but one cannot help wondering whether such a format is not rather unfair to the contributors.

H. G. ALEXANDER

Communication, Organisation, and Science. By Jerome Rothstein.

Falcon's Wing Press, Colorado, 1958. Pp. xcvi + 110. \$3.50

THE author discusses the general topics indicated in the title and those of measurement, thermodynamics, entropy, logic, language, epistemology, and brain-function, by using the concept of information as unifier. The Foreword, ninety-six pages by C. A. Muses, leaves hardly a topic untouched; but its relevance to the rest of the book is obscure.

Information and entropy are defined only vaguely, and then in the concepts of physics. An interesting attempt is made to define the entropy of a theory, taking into account its 'organisation' and its number of adjustable parameters; but the reviewer found the definitions insufficiently clear to make the concept usable. The subject of the measurement of theories is an important and fundamental one; but the author's treatment of it will have to be much stronger and more thorough before it can be handled with confidence and security.

W. ROSS ASHBY

ABSTRACTS

Philosophy of Science, 1958, **25**, No. 4

D. Riepe, 'Flexible Scientific Naturalism and Dialectical Fundamentalism'

Dialectical fundamentalism is the rigid viewpoint of many present-day philosophers of science who base their work uncritically on old-fashioned dialectical materialism. In contrast to it and also to the narrow dogmatism of logical empiricism, flexible scientific naturalism believes in the heuristic value of historical materialism joined with the analytic procedure of logical empiricism, the ethical views of scientific humanism, and suggestive elements of the ontology of recent naturalism.

V. C. Walsh, 'Scarcity and the Concepts of Ethics'

This article describes how the technical concept of 'scarcity', as taken from contemporary economic theory, can be employed in describing the cases in which persons are or are not held to be *to blame* for their actions.

W. S. Wiedorn, Jr., 'Discussion, Method in Research in Psychiatry'

Psychiatry, involving interpersonal relationship, requires participant observation as its prime research tool. The methodological importance of this is discussed.

I. M. Copi, 'The Burali-Forti Paradox'

This article is concerned with the early history of the Burali-Forti paradox, including (1) the significance of the error in Burali-Forti's first publication of the paradox, (2) the fact that the appearance of a contradiction in a mathematical theory passed almost unnoticed by mathematicians, (3) the question of priority.

Alternative formulations of the Burali-Forti paradox are presented and compared critically.

P. Moon and D. E. Spencer, 'Retardation in Cosmology'

An analysis is made of the effect of the finite velocity of light on the observed velocity and density distribution in Newtonian cosmologies.

If it is assumed that the velocity of light is constant with respect to the *source*, then this appears to accord with experiment. It also allows a universal time, even for accelerated observers, and gives Galilean relativity.

Philosophy of Science, 1959, **26**, No. 1

W. Kegley, 'Reflections on Professor Phillip Frank's Philosophy of Science'

Several questions recently raised by Professor Frank are discussed. In particular Frank's view that hypotheses are accepted in science because they make contribution to a theory which can guide human conduct. The difficulties in this viewpoint are discussed.

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A. M. Bork, 'Methodology of the Empirical Sciences'

The methodology of the empirical sciences is treated from a set-theoretical point of view. Starting from Tarski's formulation of the methodology of the deductive sciences, a relation between terms, called degree of centrality, is introduced. Epistemic correlation, and therefore the notion of interpretative system, is defined using this relation.

L. Resnick, 'Confirmation and Hypothesis'

The argument which purports to prove that many scientific hypotheses held to be *probable* are actually *certain* is critically examined. It rests on the assumption that since the non-philosopher would say of many scientific hypotheses that they are certain and would deny that the best-established hypotheses are merely probable, philosophers who say that no scientific hypotheses are certain must be mistaken. It is shown that the argument fails to take account of the technical nature of the claim that even the best-established hypotheses are probable.

J. Darlington, 'On the Confirmation of Laws'

Some difficulties involved in the application of 'degree of confirmation' to the confirmation of lawlike-statements is discussed. An alternative analysis is proposed, which is based on interval estimation. This is better able to show how instantial confirmations are inductively relevant to a law and it also requires fewer undesirable extralogical assumptions.

T. R. Williams, 'The Evolution of a Human Nature'

The development of several anthropological definitions of human nature is traced. A revised empirical definition of human nature based on other disciplines is given and leads to re-examination of paleo-anthropological data classed as unimportant under the rubrics of preceding studies. The possibility that human nature is the product of evolutionary processes is discussed.

Philosophy of Science, 1959, 26, No. 2

G. K. Herbut, 'The Analytic and the Synthetic'

It is argued that the Duhèman argument (i.e. that it is impossible to put to the test one isolated empirical statement; testing empirical statements involves testing a whole group of hypotheses) does not support the position taken by those contemporary philosophers who—like W. V. O. Quine and M. White—reject the distinction between analytic and synthetic statement.

H. M. Blalock, Jr. and A. B. Blalock, 'Toward a clarification of system analysis in the Social Sciences'

This paper attempts to outline some of the important concepts and ideas used in system analysis which is taken to be a general mode of analysis used in all sciences. Systems are seen from three perspectives: (1) that involving the relationship between system and environment, (2) that involving interaction between several systems, and (3) that involving one type of system composed of other types of systems. The writers also discuss the concepts 'structure' and 'equilibrium' as they apply to

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system analysis, the point being made that the use of these concepts in the social sciences has often been vague or even incorrect.

G. Builder, 'The Resolution of the Clock Paradox'

It is shown that the 'clock paradox' does not arise when direct use is made of the Lorentz transformation without introducing the additional, and non-essential, step in reasoning involved in utilising the clock 'rates'. It is then shown that the paradox arises only through using these clock 'rates' without due regard to the exact significance of the quantities so described; once this is recognised the paradox is resolved completely within the framework of the restricted theory, which then provides a unique and unambiguous prediction of the relative retardation. The significance of this resolution is discussed.

P. Rosen, 'The Clock Paradox and Thermodynamics'

The twin paradox of relativity theory is reviewed. A distinction is made between physical clocks and biological ones. It is suggested that metabolic activity might be a better measure of ageing than physical time. Further it is suggested that entropy changes representing metabolic activity would be a good way to describe ageing. Using the above criterion it appears that a travelling twin will be older than his brother.

Dialectica, 1958, 12, No. 1

C. Favarger, 'Vérité scientifique et compréhension du vivant'

Using botanical examples it is shown that material causality is not an adequate explanation of living things. The author discusses work of Cuénot, R. Collin, and S. Bommer, all of whose theories involve final causes.

F. Russo, 'Une perspective fondamentale de la théorie de l'information et de l'action technique: la dualité de l'action concrète et du signal'

A distinction is made between a signal and a concrete action. This distinction becomes an explanatory principle which enables complex operations to be more adequately clarified.

H. D. Rankin, 'Immediate Cognition of the Forms in the Phaedo?'

It has been maintained that the Phaedo describes apprehension of Form as being more directly dependent upon sensory perception than might be expected from a consideration of other parts of Plato's works. This paper attempts to argue that the passage in question does not involve so direct or minute a reliance upon sensory perception as critics have supposed.

Dialectica, 1959, 13, No. 1

H. König, 'Kulturbedeutung des Messens'

The author shows by examining the criteria for scientific objectivity and also the technique for forming judgments of measuring colour, that it is not possible to separate measurement and appreciation. More generally it is not possible to separate technique and culture.

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C. O. Schrag, 'Whitehead and Heidegger: Process philosophy and existential philosophy'

The philosophies of Whitehead and Heidegger display marked similarities in philosophical intention as well as in specific points of doctrine. The similarity of their definitions of philosophy, their mutual abandonment of a substance-quality metaphysics are discussed. Significant differences are also mentioned.

P. Munz, 'Investigations of Philosophy'

The practice of linguistic analysis, in so far as it is not purely lexicographical, is based on certain tacit assumptions. By its very nature such analysis precludes a discussion of the validity of these assumptions as well as a discussion of the view that there are such assumptions. It reveals itself therefore as an irrational philosophical procedure. Wittgenstein's largely valid rejection of his early positivism took the shape of linguistic analysis. But Popper's falsification criterion is a better alternative, because, unlike 'analysis' it is capable of rational discussion.

ANNOUNCEMENT

MEETINGS OF THE PHILOSOPHY OF SCIENCE GROUP

THE following meetings were held during the academic year, 1957-1958. They took place, by kind permission, in the Joint Staff Common Room, University College, London.

1957

- 14 October: Chairman's Address (Professor R. O. Kapp) 'Ockham's Razor and the Unification of Physical Science'
- 11 November: Professor S. Toulmin, 'Newton on space, time and motion: a reassessment'
- 2 December: Professor H. J. Eysenck, 'The confirmation of hypotheses in psychology'

1958

- 13 January: Dr I. J. Good, 'Could a machine make probability judgments?'
- 10 February: Professor M. Ginsberg, 'Evolution, development, progress'
- 3 March: Annual General Meeting. Mr F. Greenaway, 'The phlogiston theory and Collingwood's philosophy of History'
- 21 April: Professor A. R. Ubbelohde, F.R.S., 'Metametrics or the limits of science'
- 19 May: Mr J. W. N. Watkins, 'Confirmable and influential metaphysics'
- 16 June: Dr I. Lakatos, 'On the methodology of mathematics'

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(a) BOOKS RECEIVED FOR REVIEW

- Bunge, M., *Causality*, Harvard University Press, Oxford University Press, London, 1959, pp. xx + 380, 60s.
- Bertalanffy, L. von and Rapoport, A. (Eds.), *General Systems: Yearbook of the Society for General Systems Research*, Vol. III, University of Michigan, 1958, pp. xxiii + 259
- Briggs, M. H., *Handbook of Philosophy*, Philosophical Library, New York, 1959, pp. 214, \$4.75
- Crook, F., *Integration of Science*, Fount Books, Guernsey, 1959, pp. 99, 15s.
- Dewey, J., *Experience and Nature*, Dover Publications, Constable, London, 1959, pp. xvi + 443, 15s.
- Goussinsky, B., *Continuity and Number*, Goussinsky, Israel, 1959, pp. 31, \$0.50
- Hwang-Tsong, *The Last Principle*, Taiwan, China, 1958
- Körner, S., *Conceptual Thinking*, Dover Publications, Constable, London, 1959, pp. vii + 301, 14s.
- Martin, R. M., *The Notion of Analytic Truth*, Oxford University Press, 1959, pp. xv + 124, 40s.
- Moritz, R. E., *On Mathematics and Mathematicians*, Dover Publications, Constable, London, 1959, pp. vii + 410, 16s.
- Osler, Sir William, *A Way of Life and Other Selected Writings*, Dover Publications, Constable, London, 1959, pp. xx + 277, 12s.
- Pap, A., *Semantics and Necessary Truth*, Yale University Press, Oxford University Press, London, 1959, pp. xii + 456, 55s.
- Rehmann, G., *Das physikalische Weltbild von Morgen*, Rehmann, Dusseldorf, 1959, pp. 108, 25s.
- Reichenbach, H., *The Philosophy of Space and Time*, Dover Publications, Constable, London, 1959, pp. xvi + 295, 16s.
- Sarton, G., *A History of Science Volume 2: Hellenistic Science and Culture in the Last Three Centuries B.C.*, Harvard University Press, Oxford University Press, 1959, pp. xxvi + 554, 63s.
- Scriven, M., *La Inexistencia de la Nada*, Universidad Nacional Autonoma de Mexico, 1959, pp. 130
- Stebbing, L. Susan, *Philosophy and the Physicists*, Dover Publications, Constable, London, 1959, pp. xvi + 295, 13s.
- Stockwood, M. (Ed.), *Religion and the Scientists*, SCM Press, London, 1959, pp. 96, 5s.
- Whittaker, Sir Edmund, *From Euclid to Eddington*, Dover Publications, Constable, London, 1959, pp. ix + 212, 11s.

(b) ARTICLES

- T. A. Cowan, 'Experience and Experiment', *Philosophy of Science*, 1959, 26, 78-83
- H. Dingle, 'The Interpretation of the Special Relativity Theory' *Bull. Inst. Phys.*, 1956, 314-6

RECENT PUBLICATIONS

- H. Dingle, 'A Proposed Astronomical Test of the "Ballistic" Theory of Light Emission', *Roy. Astron. Soc.*, 1959, **119**, 67-71
- A. A. Fraeckel, 'Paul Bernays und die Begründung der Mengenlehre', *Dialectica*, 1958, **12**, 274-279
- H. Freudenthal, 'Ist die Mathematische Statistik paradox', *Dialectica*, 1958, **12**, 7-32
- J. Galtung, 'An Inquiry into the Concepts of "Reliability", "Intersubjectivity", and "Constancy"', *Inquiry*, 1959, **2**, 107-125
- F. Gonseth, 'Le Problème du langage et l'ouverture à l'expérience', *Dialectica*, 1958, **12**, 288-295
- F. Gonseth, 'L'Ouverture à l'expérience en épistémologie', *Dialectica*, 1959, **13**, 16-26
- H. Hochberg, 'Physicalism, Behaviourism and Phenomena', *Philosophy of Science*, 1959, **26**, 93-103
- J. Meynand, 'Methodological Uncertainties in Political Science', *Inquiry*, 1959, **2**, 89-106
- P. Moon and D. E. Spencer, 'Mach's Principle', *Philosophy of Science*, 1959, **26**, 125-134
- A. Moore, 'Rationalism, Empiricism, and the A Priori', *The Philosophical Quarterly*, 1959, **9**, 250-258
- J. Paiget, 'Perception, Apprentissage et Empirisme', *Dialectica*, 1959, **13**, 5-15
- F. V. Raab, 'History, Freedom and Responsibility', *Philosophy of Science*, 1959, **26**, 114-123
- C. H. Whiteley, 'Metaphysics and Science', *The Philosophical Quarterly*, 1959, **9**, 244-249